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[Intervention Review]

E-learning for health professionals

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ABSTRACT

Background

The use of e-learning, defined as any educational intervention mediated electronically via the Internet, has steadily increased among health professionals worldwide. Several studies have attempted to measure the effects of e-learning in medical practice, which has often been associated with large positive effects when compared to no intervention and with small positive effects when compared with traditional learning (without access to e-learning). However, results are not conclusive.

Objectives

To assess the effects of e-learning programmes versus traditional learning in licensed health professionals for improving patient outcomes or health professionals' behaviours, skills and knowledge.

Search methods

We searched CENTRAL, MEDLINE, Embase, five other databases and three trial registers up to July 2016, without any restrictions based on language or status of publication. We examined the reference lists of the included studies and other relevant reviews. If necessary, we contacted the study authors to collect additional information on studies.

Selection criteria

Randomised trials assessing the effectiveness of e-learning versus traditional learning for health professionals. We excluded non-randomised trials and trials involving undergraduate health professionals.

Data collection and analysis

Two authors independently selected studies, extracted data and assessed risk of bias. We graded the certainty of evidence for each outcome using the GRADE approach and standardised the outcome effects using relative risks (risk ratio (RR) or odds ratio (OR)) or standardised mean difference (SMD) when possible.

Main results

We included 16 randomised trials involving 5679 licensed health professionals (4759 mixed health professionals, 587 nurses, 300 doctors and 33 childcare health consultants).

When compared with traditional learning at 12-month follow-up, low-certainty evidence suggests that e-learning may make little or no difference for the following patient outcomes: the proportion of patients with low-density lipoprotein (LDL) cholesterol of less than 100

mg/dL (adjusted difference 4.0%, 95% confidence interval (CI) −0.3 to 7.9, N = 6399 patients, 1 study) and the proportion with glycated haemoglobin level of less than 8% (adjusted difference 4.6%, 95% CI −1.5 to 9.8, 3114 patients, 1 study). At 3- to 12-month follow-up, low-certainty evidence indicates that e-learning may make little or no difference on the following behaviours in health professionals: screening for dyslipidaemia (OR 0.90, 95% CI 0.77 to 1.06, 6027 patients, 2 studies) and treatment for dyslipidaemia (OR 1.15, 95% CI 0.89 to 1.48, 5491 patients, 2 studies). It is uncertain whether e-learning improves or reduces health professionals' skills (2912 health professionals; 6 studies; very low-certainty evidence), and it may make little or no difference in health professionals' knowledge (3236 participants; 11 studies; low-certainty evidence).

Due to the paucity of studies and data, we were unable to explore differences in effects across different subgroups. Owing to poor reporting, we were unable to collect sufficient information to complete a meaningful 'Risk of bias' assessment for most of the quality criteria. We evaluated the risk of bias as unclear for most studies, but we classified the largest trial as being at low risk of bias. Missing data represented a potential source of bias in several studies.

Authors' conclusions

When compared to traditional learning, e-learning may make little or no difference in patient outcomes or health professionals' behaviours, skills or knowledge. Even if e-learning could be more successful than traditional learning in particular medical education settings, general claims of it as inherently more effective than traditional learning may be misleading.

PLAIN LANGUAGE SUMMARY

Is e-learning more effective than traditional learning for health professionals?

What is the aim of this review?

The aim of this Cochrane Review is to find out whether e-learning, that is, interactive online educational programmes, is more effective than traditional learning (with no access to e-learning) in licensed health professionals for improving patient outcomes or health professionals' behaviours, skills and knowledge. Cochrane researchers collected and analysed all relevant evidence to answer this question and identified 16 studies.

Key messages

When compared to traditional learning, e-learning may make little or no difference for improving patient outcomes or health professionals' behaviours and knowledge, and it is uncertain whether it improves or reduces health professionals' skills.

What was studied in this review?

Modern technologies have created new platforms for advancing medical education. E-learning has gained popularity due to the potential benefits of personalised instruction, allowing learners to tailor the pace and content of courses to their individual needs, increasing the accessibility of information to remote learners, decreasing costs and facilitating frequent content updates.

Previous reviews have not identified differences, but they were limited by the type of participants included (mix of licensed health professionals and medical students) and study types evaluated (randomised together with non-randomised trials).

What are the main results of the review?

The review authors identified 16 relevant studies from 10 different countries, providing data on 5679 participants (4759 mixed health professionals, 587 nurses, 300 doctors and 33 childcare health consultants). Companies funded three studies, whereas government agencies financed six.

One study with 847 health professionals found little or no difference between e-learning and traditional learning on patient outcomes at one year, and two studies with 950 health professionals suggested little to no difference in health professionals' behaviours at 3 to 12 months, as the certainty of the evidence was low. We are uncertain whether e-learning improves or reduces health professionals' skills at 0 to 12 weeks' follow-up, based on the results of six studies with 2912 participants and very low certainty of evidence. E-learning may also make little or no difference on health professionals' knowledge, based on the results from 11 studies with 3236 participants at 0 to 12 weeks follow-up, as the certainty of the evidence was low.

How up-to-date is this review?

The review authors searched for studies that had been published up to July 2016.

SUMMARY OF FINDINGS

Summary of findings for the main comparison. Summary of findings: e-learning versus traditional learning for health professionals

E-learning versus traditional learning for health professionals

Patient or population: licensed health professionals (doctors, nurses and allied health professionals fully licensed to practice without supervision)

Settings: postgraduate education in any setting

Intervention: e-learning (any intervention in which clinical content is distributed primarily by the Internet, Extranet or Intranet)

Comparison: traditional learning (any intervention not distributed through the media mentioned above)

Outcomes	Impact*	No of participants (studies)	Certainty of the evidence (GRADE)	Comments
Patient outcomes Follow-up: 12 months	E-learning may make lead to little or no difference between the groups in proportion of patients with LDL cholesterol < 100 mg/dL (adjusted difference 4.0% (95% CI −0.3 to 7.9; 6399 patients) or glycated haemoglobin level < 8% (adjusted difference 4.6%, 95% CI −1.5 to 9.8; 3114 patients)	168 primary care clinics; 847 health professionals (1 study)	⊕⊕⊖⊖ **Low**[a]	—
Health professionals' behaviours Follow-up: 3-12 months	E-learning may make little or no difference between the groups in terms of screening for dyslipidaemia (OR 0.90, 95% CI 0.77 to 1.06, 6027 patients) or treatment for dyslipidaemia (OR 1.15, 95% CI 0.89 to 1.48; 5491 patients)	950 health professionals (2 studies)	⊕⊕⊖⊖ **Low**[b]	Studies reported multiple outcomes without specifying the primary outcome: to assess consistency, we explored 3 other possible combinations between the 2 study indicators.
Health professionals' skills Follow-up: 0-12 weeks	We are uncertain whether e-learning improves or reduces health professionals' skills (SMD 0.03, 95% CI −0.25 to 0.31, I^2 = 61%, 201 participants, 12 weeks' follow-up).	2912 health professionals (6 studies)	⊕⊖⊖⊖ **Very low**[c]	The results from the largest trial and 2 more trials, favouring traditional learning (2640 participants), and from one trial favouring e-learning could not be included in the meta-analysis. The meta-analysis included 2 trials studying different professional skills (drug dose calculation and accuracy in pressure ulcers classification).
Health professionals' knowledge Any follow-up: 0-12 weeks	E-learning may make little or no difference in health professionals' knowledge: 8 trials provided data to the meta-analysis (SMD 0.04, 95% CI -0.03 to 0.11, I^2 = 47%, 3082 participants).	3236 health professionals (11 studies)	⊕⊕⊖⊖ **Low**[d]	3 additional studies (154 participants) reported this outcome but no data were available for pooling.

CI: confidence interval; **LDL**: low-density lipoprotein; **OR**: odds ratio; **SD**: standard deviation; **SMD**: standardised mean difference.

*We interpreted SMDs using the following rules suggested by Higgins 2011a: < 0.40 represents a small effect size; 0.40 to 0.70, a moderate effect size; and > 0.70, a large effect size.

GRADE Working Group grades of evidence:
High quality: further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: we are uncertain about the estimate.

[a]Downgraded for study limitations (risk of bias and imprecision) and imprecision surrounding surrogate outcomes. Important benefits cannot be ruled out.
[b]Downgraded for study limitations (risk of bias) and inconsistency, with main effect estimates going in different directions (out of the five meta-analyses, two were in favour of e-learning and two in favour of traditional learning). Important benefits cannot be ruled out.
[c]Downgraded for study limitations: inconsistency, imprecision and indirectness. Important differences cannot be ruled out.
[d]Downgraded for study limitations (imbalance at baseline and incomplete data) and high inconsistency, with main effect estimates going in different directions (out of the eight studies, five were in favour of e-learning and three in favour of traditional learning). Although the effect estimate is imprecise, large, relevant differences are unlikely.

BACKGROUND

Description of the intervention

E-learning is a broad concept that involves the provision of educational programmes through electronic systems (Clark 2011). Currently, there is no standard or recognised definition of e-learning for research purposes. The Medical Subjects Headings Vocabulary, for example, does not provide a specific item different from 'distance education', which includes correspondence, radio and television in addition to computer networks as media tools.

For the purpose of this review, we define e-learning as any educational intervention that is mediated electronically via the Internet.

The biomedical literature contains numerous examples of terms synonymous with our definition for e-learning: web-based learning or training, online learning or education, computer-assisted or -aided instruction (CAI) or computer-based instruction (CBI), Internet-based learning (Cook 2008a; Ruiz 2006), multimedia learning, technology-enhanced learning and virtual learning. This diverse nomenclature has led to confusion: terms refer to an array of elements addressing a specific part of the e-learning concept such as the medium (e.g. computer-assisted instruction) or the delivery system (e.g. online learning). Although the term e-learning sometimes refers to blended interventions involving electronic systems and face-to-face teaching, it is generally seen as a particular evolution of distance education, that is, the use of information technologies in order to deliver education to remote learners. When these learners are computer-assisted and interconnected through computer networks, accessing online packages for learning, their distance education can unequivocally be referred to as e-learning (Ruiz 2006; Ward 2001).

How the intervention might work

Although e-learning shares many features with traditional learning systems, several aspects are unique (Zimitat 2001). Thus, assessing the quality of e-learning programmes involves more than evaluating the quality and educational design of the course content; it should also involve an analysis of navigability, multimedia approach, degree of interactivity, and other key factors like intervention duration, repetition and feedback or layout impact in the development of an optimal e-learning framework (Cook 2010a; Menon 2012; Straus 2004). The traditional role of trainers is evolving from a 'distributor of content' to a 'facilitator', enhancing the learner-centred characteristics of the educational programme (Wentling 2000).

Applying the latest information technologies to education takes advantage of the increasing availability of Internet access (via optical fibres, WiFi and 3G/4G mobile phone technology), allowing a broad use of content across diverse settings (home, workplaces, and public places such as libraries, parks, and Internet points).

The delivery advantages of an e-learning programme are obvious: some of their most cited benefits include lower costs, widespread distribution, increased accessibility to information, frequent content updates and personalised instruction in terms of content and pace of learning (Wentling 2000). Moreover, the interactivity and ability to link educational programmes with past experiences and specific needs fit the adult learning paradigm (Gibbons 2000).

As a result of these advantages, online learning is becoming more popular, and online courses worldwide are rapidly increasing in number, offering many specialty modules in their portfolios (Coppus 2007; Moja 2007; Ruiz 2007). Potential disadvantages include technology-related costs, cost involved in developing programmes, possible technical problems, limited direct interaction, lack of exchanges and relations with other learners, absence of the physical presence of the teacher, decrease in motivation to learn, need for greater self-discipline, and attenuation of the desire to compete with other learners (Cook 2007; Poon 2015; Welsh 2003). Moreover, equity should be considered carefully: poor access, language barriers, and lack of computer and Internet literacy could limit or prevent the participation of some health professionals, especially in low- and middle-income countries. These limitations might prevent e-learning from becoming the norm.

Previous systematic reviews on the efficacy and efficiency of e-learning focused on the outcomes laid out in Kirkpatrick 1996: satisfaction, knowledge/attitudes, skills (in a test setting), behaviours (in a practice setting) and effects on patients (Cook 2008a; Cook 2010a; Lahti 2014; Lam-Antoniades 2009; Sinclair 2016). Knowledge measurement by standardised tests is the most common outcome for both traditional and e-learning systems. However, the progression from cognitive to behavioural steps – from acquiring knowledge to performing a task in practice – is neither linear nor simple: many other factors influence health professionals' behaviours, including system-related factors (e.g. government incentives, guidelines, laws) and individual-related factors (e.g. patient expectations, relationship with peers) (Rethans 2002).

These reviews found:

- e-learning is associated with large positive effects when compared with no intervention (Cook 2008a);
- e-learning is associated with small positive effects when compared with traditional educational interventions (without access to e-learning), suggesting similar effectiveness (Cook 2008a; Lahti 2014; Sinclair 2016);
- e-learning and traditional educational interventions take similar time to participate in or complete (Cook 2010c);
- insufficient evidence is available comparing e-learning and traditional educational interventions on licensed health professionals' behaviours and patient outcomes (Sinclair 2016)
- interactivity, practice exercises, repetition and feedback play pivotal roles in e-learning and seem to be associated with improved learning outcomes (Cook 2010a).

A further relevant finding was the large heterogeneity in study designs, participants, instructional designs and outcomes. The authors conclude that e-learning is not a single entity, although educators and researchers frequently view it as a single activity or a cluster of single activities, with relatively homogeneous effects (Cook 2010b).

Why it is important to do this review

E-learning is gaining in popularity, and programmes are rapidly increasing in number. Their relatively low costs, high flexibility, and reduced dependence on geographical or site boundaries are attracting the investments of stakeholders (countries, networks,

and universities) and increasing the demands of learners. This review synthesises the evidence for the effectiveness of e-learning versus traditional educational interventions for licensed health professionals: more precise data about the effectiveness of e-learning programmes have the potential to influence future investments regarding continuing medical education (CME) programmes.

OBJECTIVES

To assess the effects of e-learning programmes versus traditional learning in licensed health professionals for improving patient outcomes or health professionals' behaviours, skills and knowledge.

METHODS

Criteria for considering studies for this review

Types of studies

We included randomised trials and cluster-randomised trials.

We used the Cochrane definitions for randomised trials (Higgins 2011a).We excluded non-randomised trials (e.g. controlled before-after studies or interrupted time series) as they are prone to a wider range of potential risks of bias and add little value when sufficient evidence is available from randomised trials (EPOC 2013a). Non-randomised quality-improvement intervention trials often overstate the strength of causal inference between intervention and outcomes compared to randomised trials (Li 2009). Conclusions from meta-analyses exploring the causality of e-learning might be undermined if largely based on studies that adopt intrinsically weaker research designs (Banzi 2009).

We included studies published in all languages and providing data about any follow-up periods.

Types of participants

We included studies assessing e-learning programmes aimed at improving patient outcomes or behaviours, skills or knowledge of licensed health professionals (doctors, nurses and allied health professionals). We focused on the license to practice without supervision as a discriminating factor, that is, health professionals who can fully practice a specific health-related profession versus those who cannot. We included only those licensed to practice in this review. If the description was not sufficient, we sent requests to the study authors for additional information before excluding the studies.

We excluded studies recruiting undergraduate students, trainees and residents, or a mix of licensed and unlicensed participants, if data on the eligible participants were not provided by the authors after a formal request by email.

Types of interventions

Definition of e-learning programme

We included any intervention distributing and facilitating access to clinical content primarily by the Internet, Extranet or Intranet: web-based tutorials, virtual clinical vignettes, online discussion groups, Internet-mediated videoconferencing, web seminars, emails, podcasts and virtual social networks. We excluded CD-ROMs and applications not distributed through the media mentioned above. The learners may have had access to interventions through a variety of technologies (e.g. computers, personal digital assistant (PDA), smart phones, etc). We applied no restrictions with regard to the programme length: we included short programmes such as single lectures, workshops and modules as well as more extended educational programmes. We included an intervention if the description was sufficient to allow us to establish whether it could potentially improve knowledge or behaviours by any kind of intervention mentioned above; we also included interventions if the description was sufficient to allow us to establish that it was aimed at improving clinical practice (starting effective treatment or dismissing ineffective or harmful treatment). On the contrary, if the description proved unclear or insufficient, we sent a request to the study authors for additional information before excluding the studies.

We excluded e-learning programmes focusing on non-clinical medical topics (e.g. bio-terrorism), defined as subjects different from the seven roles that all physicians need to have to be better doctors: medical expertise, communication, collaboration, leadership, health advocacy, scholarship and professionalism (The CanMEDS Framework).

We only included interventions in which e-learning is a core or essential element. However, in multifaceted educational interventions (e.g. those applying two or more interventions to change health professionals' practice), the e-learning component may have different degrees of centrality. Thus, we categorised studies into three groups:

1. e-learning alone;
2. e-learning as a core, essential component of a multifaceted intervention;
3. e-learning as a component of a multifaceted intervention, but not considered core and essential.

We classified studies as having 'core' e-learning interventions when e-learning was the main part of the educational intervention (e.g. e-learning together with the dissemination of guideline in a paper format). When learners could use the components other than e-learning in the absence of e-learning, or e-learning was merely added to a multifaceted intervention that could easily be offered in its absence (e.g. audit and feedback interventions), we considered the intervention as 'not core'.

We included trials where the eligible comparators were educational interventions on the same topic without access to e-learning (e.g. print books, face-to-face residential courses, guidelines dissemination) or multifaceted educational interventions without e-learning on the same topic.

Types of outcome measures

We included the following outcome measures: patient outcomes and health professionals' behaviours, skills or knowledge (Kirkpatrick 1996; Straus 2004).

For the purposes of this review, we assessed different components targeted by educational interventions in clinical practice, excluding subjectively assessed outcomes (e.g. learner satisfaction or self-reported knowledge, intentions to do, or beliefs about capabilities).

1. Patient outcomes defined as occurrence of deaths (i.e. mortality) or illness (i.e. morbidity; e.g. pneumonia, myocardial infarction, stroke) or progression of disease or hospitalisation.
2. Health professionals' behaviours, defined as actual professional performance: the incorporation of knowledge and skills into practice, with the adoption of proven treatments and interventions that can potentially improve patients' health.
3. Health professionals' skills, defined as deep learning or competence (what the learner is able to do), for example posing structured clinical questions considering patients, treatments, comparisons and outcomes, and understanding quantitative aspects (e.g. relative or absolute risk reduction, number needed to treat).
4. Health professionals' knowledge defined as factual knowledge or basic learning, for example knowing the benefits and risks of different interventions (e.g. in patients with unstable angina, aspirin is beneficial).

Primary outcomes

Patient clinical outcomes

- Any objective measure of patient clinical outcomes (e.g. blood pressure, number of caesarean sections, medical errors)

Health professionals' behaviour

- Any objective measure of clinical performance (e.g. number of tests ordered, prescriptions for a particular drug).

We assessed primary outcomes at two major time points:

1. immediately after the e-learning intervention; and
2. at the longest duration of follow-up available.

Secondary outcomes

Skills and knowledge are clinical competence dimensions related to the concept of 'know' (knowledge) and 'know-how' (skills) (Miller 1990).

Health professionals' skills

- Any objective measure of skills such as the assessment of learners' ability to demonstrate a procedure or technique (e.g. problem solving, objective structured clinical examination scores)

Health professionals' knowledge

- Any objective measure of learners' knowledge such as assessment of factual or conceptual understanding (e.g. multiple-choice test of knowledge).

Search methods for identification of studies

Electronic searches

The EPOC Information Specialist wrote the search strategies in consultation with the authors. We searched the Cochrane Database of Systematic Reviews (CDSR) and the Database of Abstracts of Reviews of Effects (DARE) (via the Cochrane Library) for related systematic reviews, and the following databases for primary studies:

- Cochrane Central Register of Controlled Trials (CENTRAL; 2016, Issue 6) via Wiley (searched 7 July 2016).
- MEDLINE, Epub Ahead of Print, In-Process & Other Non-Indexed Citations, MEDLINE Ovid Daily and MEDLINE Ovid, OvidSP (1946 to 7 July 2016).
- Embase OvidSP (1980 to 7 July 2016).
- Health Technology Assessment (2016, Issue 2) via Wiley (searched 7 July 2016).
- NHS Economic Evaluation Database (2016, Issue 2) via Wiley (searched 7 July 2016).
- Database of Abstracts of Reviews of Effects (2016, Issue 2) via Wiley (searched 7 July 2016).

Search strategies are comprised of keywords and controlled vocabulary terms. We applied no language or time limits. All strategies used are provided in Appendix 1

Searching other resources

We searched the following trial registries for ongoing and completed trials.

- Word Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP) (www.who.int/ictrp/en).
- ClinicalTrials.gov, US National Institutes of Health (NIH).

We examined the reference lists of the included trials and relevant reviews published in the field of e-learning (e.g. Chumley-Jones 2002; Cook 2008a; Lam-Antoniades 2009; Ruiz 2006; Wentling 2000; Wutoh 2004).

Data collection and analysis

Two review authors independently determined the eligibility of the intervention by examining the study report and the description of the intervention. If necessary, we referred to other related papers or reports (e.g. protocol or register records) and sent requests to the study authors for additional information, especially if e-learning programmes were unclear or trialists did not clearly report the measures to monitor outcomes changes.

We collated multiple reports of the same studies so that each study, rather than each report, was the unit of interest in the review.

Where means and standard deviations (SDs) were not reported in the original article, we sent requests to the study authors for additional information.

We examined any relevant retraction statements and errata, and we searched for any key unpublished information that was missing from the reports of the included studies.

We used Review Manager 5 (RevMan 5) software to manage the included studies data (RevMan 2014).

Selection of studies

Two review authors independently screened the titles and abstracts and applied inclusion and exclusion criteria. We searched for complete manuscripts in the cases of uncertainty and resolved disagreements through discussion and consensus.

We documented the studies selection process in a PRISMA flow diagram (Liberati 2009).

Data extraction and management

Two review authors independently extracted data from the included studies, using a data sheet based on a modified version of the EPOC data collection checklist (EPOC 2015).

We extracted the following information.

1. Characteristics of participants: total number at baseline, total number at completion of the study, and type of target health professionals.
2. Interventions and controls: number of groups, interventions applied, frequency, duration and main components.
3. Methods: study design, duration of the study, setting and provider.
4. Outcomes: type of outcome measures, scales of measure, values for means and standard deviations.
5. Results: measures at follow-up (including means and SD/ standard errors (SEs)/confidence intervals (CIs) for continuous data and summary table for dichotomous data), withdrawals and loss to follow-up.

We resolved any disagreement by discussion to reach a consensus. We described any ongoing study, if available, detailing its primary author, research question, methods and outcome measures along with its estimated date of completion.

Assessment of risk of bias in included studies

Two review authors independently assessed the quality of all eligible studies using the EPOC risk of bias criteria (EPOC 2013b). We resolved any discrepancies in quality rating by discussion and consensus. We collected the sources of information (to support our judgments) for each risk of bias assessment (e.g. quotation, summary of information from trial reports, correspondence with investigators). For each study, we assessed the following nine standard criteria for risk of bias.

1. Was the allocation sequence adequately generated?
2. Was the allocation adequately concealed?
3. Were baseline outcome measurements similar?
4. Were baseline characteristics similar?
5. Were incomplete outcome data adequately addressed?
6. Was knowledge of the allocated interventions adequately prevented during the study?
7. Was the study adequately protected against contamination?
8. Was the study free from selective outcome reporting?
9. Was the study free from other risks of bias?

We summarised the overall risk of bias for the single studies, considering the risk of bias for allocation concealment, incomplete outcome data, and blinding of outcome assessors to be key domains (Chan 2004; Dwan 2008; Kirkham 2010; Savovic 2012; Wood 2008). We judged the overall risk of bias at study level to be high if we had rated one of these items as being at high risk of bias and as low if we had judged all the items to be at low risk. We used the risk of bias of the single studies in the sensitivity analysis as detailed below.

Measures of treatment effect

We separately analysed patient outcomes, health professionals' behaviours, skills and knowledge.

When possible, we calculated the outcome measures in accordance with the intention-to-treat principle (i.e. analysing all data according to randomised group assignment, regardless of whether some of the participants violated the protocol, failed to adhere or were lost to follow-up). Accordingly, we contacted study authors to obtain additional primary trial data when necessary.

We based analyses on the consideration of dichotomous (e.g. proportion of patients managed according to e-learning programme) or continuous process measures (e.g. change in learners' knowledge scores). Where studies reported more than one measure for each endpoint, we planned to abstract the primary measure (as defined by the study authors) or the median measure identified. For example, if the comparison reported five continuous knowledge test variables and none of them were denoted as the primary variable, we ranked the effect sizes for the five variables and took the median value.

We extracted the outcomes from each study in natural units. We planned to combine final values if all the studies used the same scale, convert the effect size back into the natural units of the outcome measure most familiar to the target audience, or provide a standardised effect size.

We only included continuous data from a trial in the analyses if:

1. means and SDs were available or could be calculated; and
2. there was no clear evidence of a skewed distribution (e.g. as indicated by the ratio between the difference between the minimum or maximum value of the scale and the SD (Deeks 2011).

Because final value and change scores from baseline to final values should not be combined together as standardised mean difference, for studies providing both measures of treatment effect for continuous outcomes, we privileged the post-test means. Due to randomisation, we did not expect differences between experimental and control group baseline scores (Higgins 2011a).

We planned to use results from both periods of cross-over trials, unless there was a risk of carryover effects from one period to another, which presents a serious flaw. For cross-over trials, we planned to use paired estimates of the effect (e.g. means and its SE), or calculated them from the exact statistical test results (e.g. paired t-test for continuous data or McNemar's test for binary outcomes) (Cook 2008a; Elbourne 2002).

We present binary outcomes using odds ratios (OR) as appropriate and their 95% confidence intervals. For continuous outcomes, we report mean and standard deviation SD and standardised mean differences (SMD) for studies evaluating the same outcome in different ways. We interpreted the magnitude of the SMD as small for values of about 0.2, medium for SMDs of 0.5, and large for SMDs of 0.8 or more (Cohen 1988).

Unit of analysis issues

Studies with more than two arms

If more than one comparison from a study with more than two arms was eligible for the same comparison, we planned to adjust the number of health professionals to avoid double counting. We sought to make the adjustment by dividing the number of health professionals in the shared arm more or less evenly among the comparisons.

Cluster-randomised trials

Owing to the focus on an educational intervention, we expected trials to be randomised by groups of professionals. In cluster-randomised trials, 'clusters' of individuals are randomly allocated to study arms, and investigators measure outcomes based on the individual cluster members. Under such circumstances, it is necessary to adjust the results from primary trials for clustering before they are included in the meta-analysis in order to avoid spurious precision in 95% CIs. We included cluster-randomised trials with adequate definition of participants and clusters, as suggested by the Ottawa Statement for cluster-randomised trials (Weijer 2012).

For the cluster-randomised trials, in order to calculate adjusted (inflated) CIs that account for the clustering, we planned to proceed to an approximate analysis. Our approach was to multiply the SE of the effect estimate (from the analysis ignoring the clustering) by the square root of the design effect. For this, we used intra-correlation coefficients borrowed from an external source (University of Aberdeen 2015).

Performing meta-analyses using studies with unit of analysis errors required us to make a number of assumptions about the magnitude of unreported parameters, such as the intra-correlation coefficients and the distributions of patients across clusters. We planned to re-analyse studies with potential unit of analysis errors where possible, reporting the re-analysed results (observed SEs, P values, or CIs) in an additional table along with the original results. If this was not possible, we reported only the original results and excluded the study from the meta-analyses.

Dealing with missing data

For all included studies, we analysed available data obtained either from publications or following correspondence with the authors. In the Discussion section of the review, we considered the extent to which the missing data could alter our results and conclusions.

For all outcomes across all studies, we carried out analyses as far as possible on an intention-to-treat basis (i.e. we attempted to include all participants randomised to each group in the analyses, regardless of whether or not they received the allocated intervention). If intention-to-treat data were not available or for dichotomous and continuous data that were missing, we made no assumptions about loss to follow-up, but we based analyses on participants completing the trial. If there was a discrepancy between the number randomised and the number analysed in each treatment group, we calculated and reported the percentage of loss to follow-up in each group.

Where standard deviations were not specified, we calculated them using the exact statistical test results (e.g. P value related to t or F statistic) or, if these were not reported, we used differences in change scores, standardised using pretest variance. If neither P values nor any measure of variance were reported, we planned to use the average standard deviation from other similar studies (Cook 2008a).

We considered the impact of missing data separately for each primary and secondary outcome reported in each study.

Assessment of heterogeneity

To assess the contextual heterogeneity of the included trials (the differences in populations, context, interventions, comparators, follow-up), we planned to conduct subgroup analyses according to important clinical and methodological characteristics, such as settings, interventions, comparators, etc. Between-study heterogeneity was planned to be assessed overall and within the subgroups.

We included all the pre-specified outcomes available from the individual studies in the meta-analysis, with heterogeneity reported by the Q (Chi^2) and the I^2 statistics (Deeks 2011). The I^2 describes the percentage of the variability in effect estimates that is due to heterogeneity rather than chance (sampling error). The *Cochrane Handbook for Systematic Reviews of Interventions* gives the following guidance on this decision based on I^2 values to classify the inconsistency of the effect measures across studies (Higgins 2011a).

- 0% to 40%: might not be important.
- 30% to 60%: may represent moderate heterogeneity.
- 50% to 90%: may represent substantial heterogeneity.
- 75% to 100%: considerable heterogeneity.

In cases of moderate/substantial heterogeneity, we performed the analysis using both the fixed-effect and the random-effects model. Where considerable heterogeneity existed, we explored the magnitude and direction of the effects: if I^2 was more than 75%, but the large majority of effect estimates were in the direction of benefit, and a random-effects meta-analysis yielded highly statistically significant benefits, we accepted the results. In this scenario, there would be some uncertainty about the amount of benefit but not its existence; it is safe to conclude that the intervention is beneficial (Virgili 2009). If substantial heterogeneity existed, studies were sparse or directions discordant, we did not pool data from the trials, and we did not conclude in favour of or against the intervention.

Assessment of reporting biases

We planned to use funnel plots to assess the reporting biases. We planned to evaluate the funnel plot asymmetry, not only visually but also with the use of tests for funnel plot asymmetry if we found more than 10 studies to include in the meta-analysis. We planned to use the test proposed by Egger 1997 and by Harbord 2006 for continuous and dichotomous outcomes, respectively. If we detected asymmetry, we discussed possible explanations (e.g. publication bias or poor methodological quality of the studies) on the basis of available information and subsequently performed a sensitivity analysis (Higgins 2011b). We interpreted funnel plots cautiously, as they may be misleading.

Data synthesis

We grouped the studies according to important clinical and methodological (conceptual) characteristics, such as settings, interventions, comparators, etc. Accordingly, we synthesised similar studies reporting homogeneous (similar) outcomes and outcome measures.

We entered outcomes into RevMan 5 as effect sizes and their SEs (RevMan 2014).

We conducted meta-analyses using both random-effects and fixed-effect models.

Subgroup analysis and investigation of heterogeneity

We planned to perform the following subgroup analyses if at least 10 observations (i.e. 10 studies in a meta-analysis) were available for each characteristic modelled (Higgins 2011a).

- Content: e-learning programmes subgrouped by medical, surgical or rehabilitation topics, with the hypothesis that e-learning programmes about medical topics (more likely to be centred on knowledge than skills or behaviours) are more effective than e-learning programmes focused on other topics.
- Health professionals targeted: doctors, nurses or physiotherapists, with the hypothesis that e-learning programmes for doctors are more effective than e-learning programmes for other health professionals.
- Regulation: formally accredited versus non-accredited e-learning programmes, with the hypothesis that accredited e-learning programmes are more effective than non-accredited ones.
- Format:
  - high-interaction programmes (combination of at least three components, e.g. web module, chat, emails) or low-interaction programmes (fewer than three components), with the hypothesis that high-interaction programmes are more effective;
  - short (i.e. less than one week in duration) or long programmes (more than one week in duration), with the hypothesis that short programmes are more effective.

Other authors have identified some of these factors as potentially influencing the effect of educational e-learning programmes (Cook 2008a; Cook 2008b; Ruiz 2006). We undertook the standard test for heterogeneity across subgroup results to investigate the differences between two subgroups (Borenstein 2009). We used these analyses to investigate potential sources of heterogeneity and reported them as post hoc exploratory data analyses only.

Sensitivity analysis

We planned to perform sensitivity analyses:

- excluding studies assessed as at high risk of bias; and
- excluding cross-over trials.

We decided to aggregate studies at unclear risk of bias to those at high risk of bias. We adopted a conservative approach, assuming that an absence of information indicated inadequate quality ('guilty until proven innocent') (Moja 2014).

Summary of findings table

We assessed the certainty of evidence for pre-specified outcomes using GRADEpro software (GRADEpro 2008). We justified all decisions to downgrade or upgrade the rating using footnotes, and we provided comments to aid readers' understanding of the review when necessary, as recommended by Cochrane (Schünemann 2011). Summary of findings for the main comparison includes the overall grading of the certainty of evidence related to each of the outcomes according to the GRADE approach. We graded the certainty of evidence as high, moderate, low or very low; we downgraded the initial level of confidence considering the risk of bias, inconsistency and indirectness of evidence, imprecision of effect estimates and risk of publication bias.

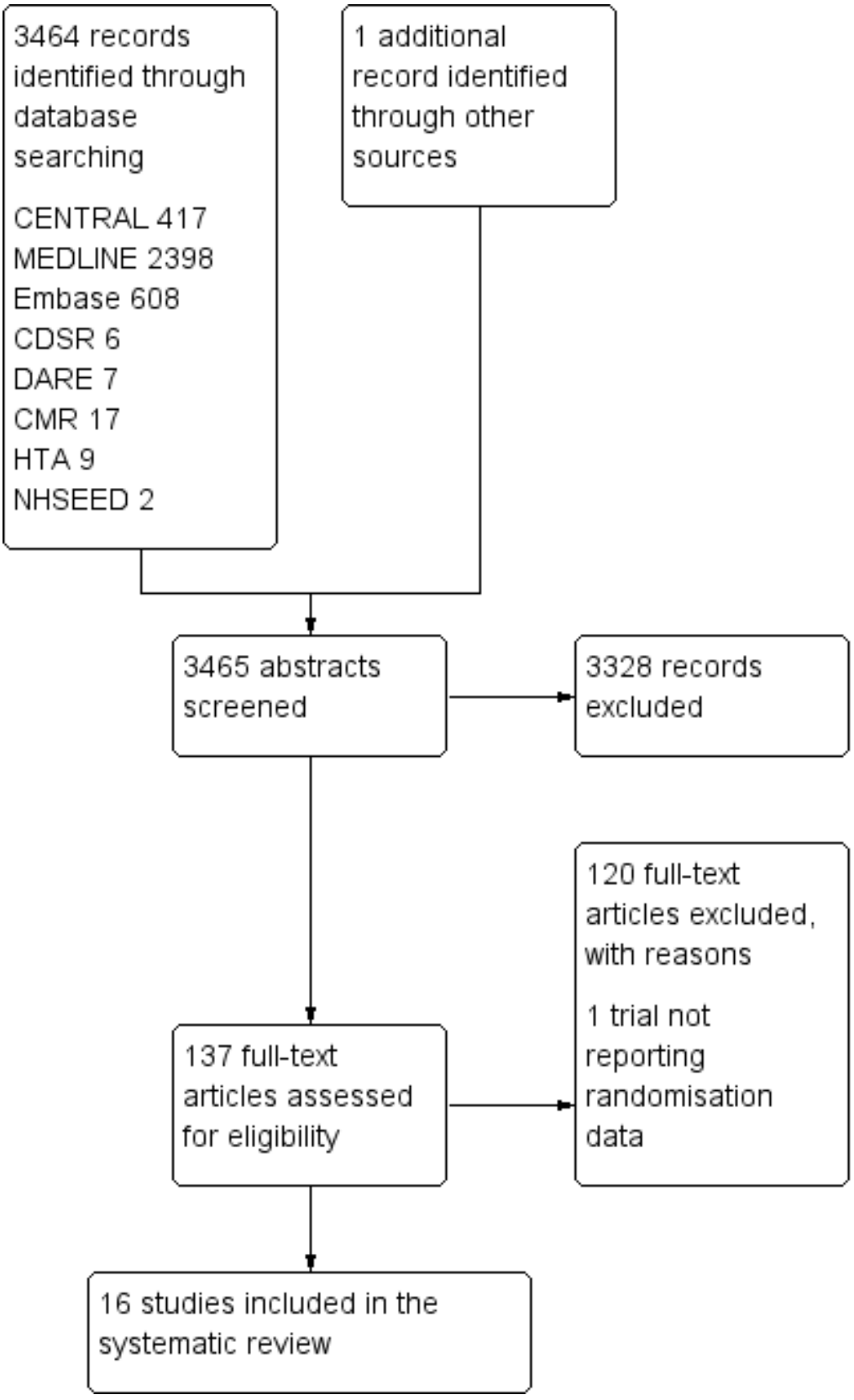
RESULTS

Description of studies

Results of the search

We identified 3464 records through the search strategy (CENTRAL 417, MEDLINE 2398, Embase 608, CDSR 6, DARE 7, CMR 17, HTA 9, NHSEED 2) and one additional article from other reviews. We excluded 3328 articles based on the abstracts (Figure 1).

Figure 1. Study flow diagram



We retrieved the full text of 137 articles to determine their eligibility for inclusion, excluding 121 records and including 16.

Included studies

Sixteen randomised trials providing data on 5679 learner participants met our predefined selection criteria. The trials were all published between 2005 and 2016. The mean sample size was 400 participants, but only 3 trials had more than 150 participants. Six trials took place in the USA (Benjamin 2008; Fordis 2005; Harris 2008; Le 2010; Levine 2011; Wilson-Sands 2015), while the remaining 10 studies were in Japan (Horiuchi 2009), the Netherlands (Hugenholtz 2008), Finland (Mäkinen 2006), Australia (Maloney 2011; Perkins 2012), Brasil (Paladino 2007), the UK (Perkins 2012), Taiwan (Sheen 2008), Norway (Bredesen 2016; Simonsen 2014), and Iran (Khatony 2009); only Perkins 2012 was performed in two countries.

Characteristics of participants and settings

Four trials randomised 4759 mixed health professionals (Levine 2011; Maloney 2011; Perkins 2012; Wilson-Sands 2015), seven trials randomised 587 nurses (Bredesen 2016; Horiuchi 2009; Khatony 2009; Mäkinen 2006; Paladino 2007; Sheen 2008; Simonsen 2014), four trials randomised 300 doctors (Fordis 2005; Harris 2008; Hugenholtz 2008; Le 2010), and one trial randomised 33 childcare health consultants (Benjamin 2008). Four trials took place in a primary care setting (Fordis 2005; Harris 2008; Le 2010; Levine 2011), six trials in a secondary care hospital setting (Horiuchi 2009; Khatony 2009; Mäkinen 2006; Paladino 2007; Sheen 2008; Wilson-Sands 2015), three trials in a mixed setting (Bredesen 2016; Perkins 2012; Simonsen 2014), and one in a rehabilitation setting (Maloney 2011). Two trials were performed in other settings (Benjamin 2008; Hugenholtz 2008).

Characteristics of educational interventions used in the trials

All 16 trials included in our review compared e-learning interventions versus face-to-face residential learning except for two trials comparing e-learning with guideline dissemination or availability (Le 2010; Levine 2011). In five trials, the educational intervention was accredited for CME purposes (Fordis 2005; Harris 2008; Hugenholtz 2008; Le 2010; Levine 2011). In six trials, the duration of the e-learning intervention, in terms of time needed to be spent on learning, was the same as the control intervention (Harris 2008; Hugenholtz 2008; Levine 2011; Maloney 2011; Perkins 2012; Simonsen 2014); in three trials, the duration of the educational session was longer in the control groups than in the e-learning groups (Horiuchi 2009; Mäkinen 2006; Paladino 2007); in the remaining cases, investigators did not describe this information or confused it with the time the intervention was available to the participants. We considered the amount of time needed to be spent on learning as short (less than one week) in all trials except in Le 2010 and Levine 2011. In 11 trials e-learning was administered alone, not in combination with other interventions; in the 5 remaining trials (Fordis 2005; Le 2010; Levine 2011; Maloney 2011; Perkins 2012), we considered e-learning as being a core and essential element of a multifaceted educational intervention. The interactivity of the e-learning tools was high (combination of at least three components) in nine trials and low in seven trials (Bredesen 2016; Harris 2008; Horiuchi 2009; Hugenholtz 2008; Paladino 2007; Sheen 2008; Wilson-Sands 2015).

Outcome assessment

Investigators assessed patient outcomes by analysing administrative data; health professionals' behaviours, by auditing patients' charts and analysing administrative data and health professionals' skills, by administering written skills tests, simulations or objective structured clinical examinations. Trials assessed the 'knowledge' outcome through questionnaires: in four trials, the authors reported that the questionnaire was previously validated (Fordis 2005; Harris 2008; Khatony 2009; Perkins 2012), while the other studies did not specify.

Duration of follow-up and outcome assessment times

The median follow-up time from the conclusion of the educational intervention to the last outcome assessment was 1.5 weeks, ranging from 0 to 52 weeks. During the study, only three trials had more than one outcome assessment (Fordis 2005; Harris 2008; Le 2010).

For additional details on the studies, please refer to the Characteristics of included studies table.

Excluded studies

We excluded 121 studies for the following reasons: control group (no intervention at all, intervention on a different topic or different types of e-learning in the control group), 51 studies; type of participants included (students or trainees), 30 studies; study design (non-randomised trials), 21 studies; type of intervention used (not e-learning, not delivered by the Internet, not core and essential or not compliant with CanMEDS criteria), 12 studies; type of outcome assessed (no outcome of interest or self-reported outcome), 6 studies; incompleteness of data concerning the number of participants randomised per group, as well as the authors' inability to answer our request for clarification, 1 study (Esche 2015).

For additional details on the studies refer to the Characteristics of excluded studies table.

Ongoing trials

We did not identify any ongoing trials.

Risk of bias in included studies

We summarised decisions regarding individual domains within the Cochrane 'Risk of bias' tool in the 'Risk of bias' graph (Figure 2) and summary (Figure 3). We provided full details of review authors' judgments and support for judgments for each study within the 'Risk of bias' tables in the Characteristics of included studies.

Figure 2. Risk of bias graph: review authors' judgments about each risk of bias item presented as percentages across all included studies.

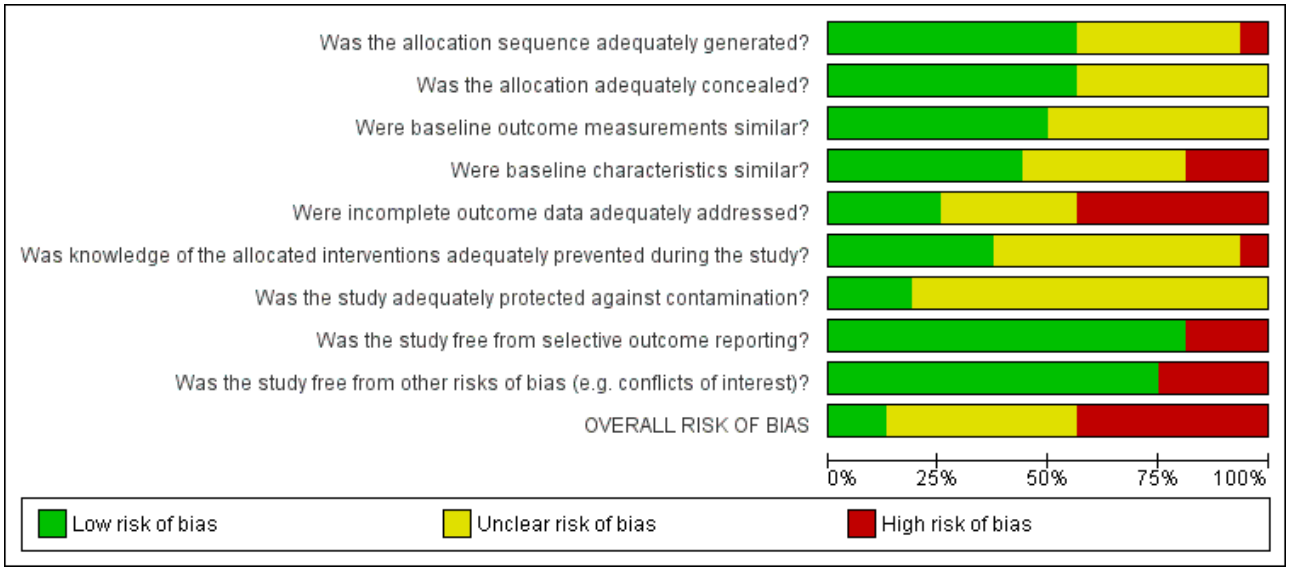


Figure 3. Risk of bias summary: review authors' judgments about each risk of bias item for each included study.

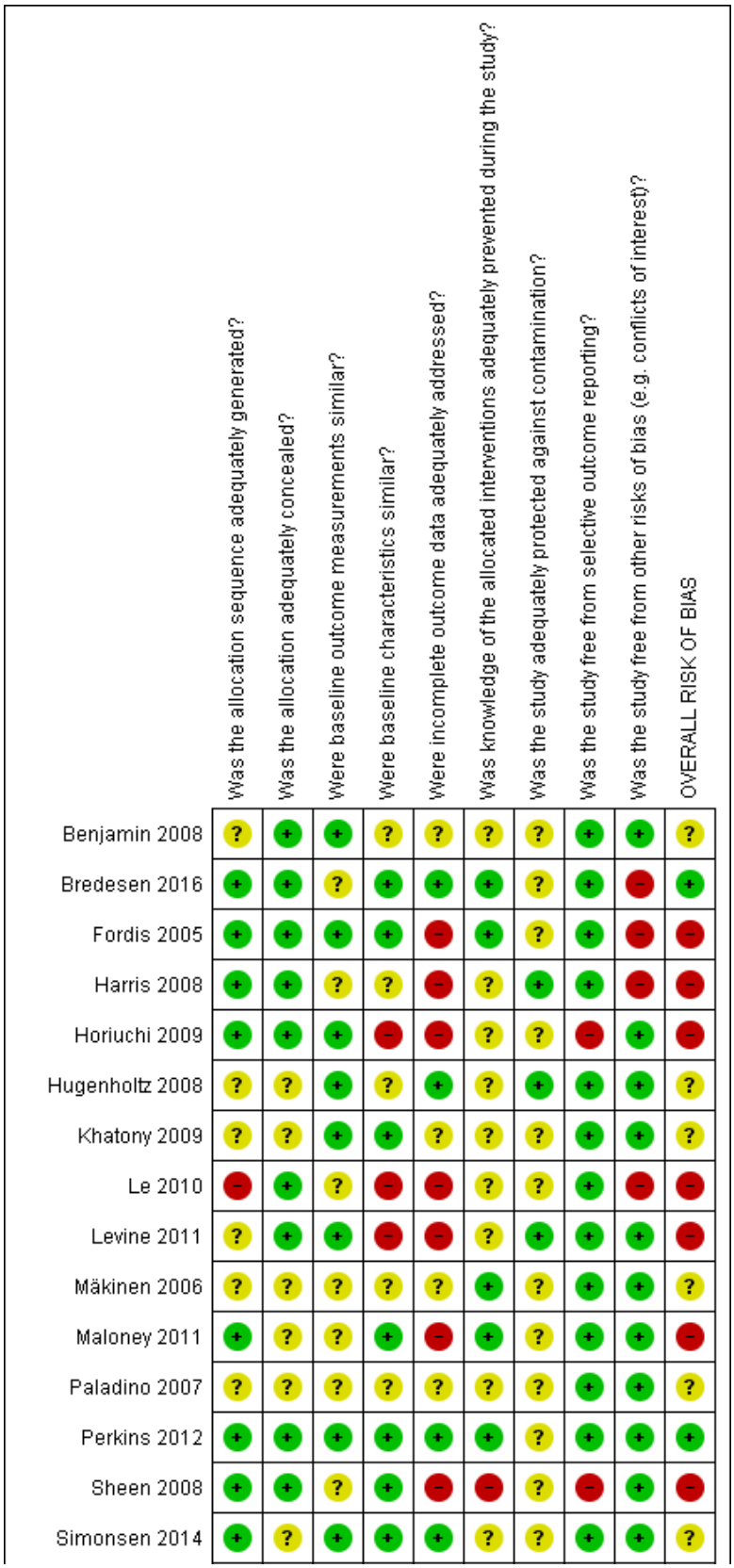
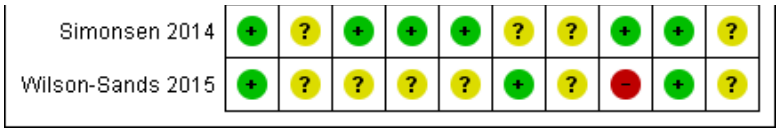


Figure 3. (Continued)



Was the allocation sequence adequately generated?

Nine studies used acceptable methods to generate the allocation sequence, including computerised random number generators (Fordis 2005; Horiuchi 2009; Maloney 2011; Perkins 2012; Simonsen 2014), a blind name draw (Harris 2008), a coin flip (Sheen 2008), or card or envelope shuffling (Bredesen 2016; Wilson-Sands 2015); the remaining trials were at unclear risk of bias with the exception of one study that was at high risk of bias as participants from the same practice were matched into pairs before randomisation (Le 2010).

Was the allocation adequately concealed?

Nine studies clearly explained how the sequence was concealed (Benjamin 2008; Bredesen 2016; Fordis 2005; Harris 2008; Horiuchi 2009; Le 2010; Levine 2011; Perkins 2012; Sheen 2008), while the remaining ones did not mention the methods used by the investigators.

Were baseline outcome measurements similar?

Eight studies clearly reported similar baseline outcome measurements (Benjamin 2008, Fordis 2005, Horiuchi 2009, Hugenholtz 2008, Khatony 2009, Levine 2011, Perkins 2012, Simonsen 2014). We considered the remaining studies at unclear risk of bias because they did not report any information.

Were baseline characteristics similar?

Seven studies reported similar baseline characteristics (Bredesen 2016, Fordis 2005, Khatony 2009, Maloney 2011, Perkins 2012, Sheen 2008, Simonsen 2014) and six were unclear (Benjamin 2008, Harris 2008, Hugenholtz 2008, Mäkinen 2006, Paladino 2007, Wilson-Sands 2015); we considered three trials at high risk of bias because of unbalance in the participants baseline characteristics (Horiuchi 2009, Le 2010, Levine 2011).

Were incomplete outcome data adequately addressed?

We judged seven studies to be at high risk of attrition bias (Fordis 2005; Harris 2008; Horiuchi 2009; Le 2010; Levine 2011; Maloney 2011; Sheen 2008): Sheen 2008 used a per-protocol analysis, and the remaining six studies reported high loss to follow-up, ranging from 15% in Fordis 2005 to 47% in Levine 2011. In four out of these studies, the attrition was bigger in the e-learning group than in the control group (Fordis 2005; Harris 2008; Le 2010; Maloney 2011). We also judged four studies to be at low risk of attrition bias (Bredesen 2016; Hugenholtz 2008; Perkins 2012; Simonsen 2014), while five did not specify anything about loss to follow-up (Benjamin 2008, Khatony 2009, Mäkinen 2006, Paladino 2007, Wilson-Sands 2015).

Was knowledge of the allocated interventions adequately prevented during the study?

Participant blinding is not feasible in educational studies, so performance bias might be unavoidable in this setting. We considered the blinding of assessors, rating the risk of detection bias as high in Sheen 2008 because the authors clearly stated that the assessors were not blind. The study was so small that the assessors could possibly know and remember participants' allocation. Also in Perkins 2012, the authors were unable to ensure the blinding of the outcome assessors. However, this study was so large that we assumed some degree of separation between participants and assessors; besides, the process of measurement was well structured, limiting the risk of bias. Four studies reported that the knowledge of the allocated interventions was adequately prevented (Bredesen 2016, Fordis 2005; Mäkinen 2006; Maloney 2011) and we considered these studies as having low risk of bias. The remaining studies did not report any information on the blinding of the outcome assessors.

Was the study adequately protected against contamination?

Only three trials were clearly reported with respect to the protection against contamination (Harris 2008, Hugenholtz 2008, Levine 2011) while all the others were unclear.

Was the study free from selective outcome reporting?

We found inconsistencies between the outcomes declared in the methods section and the outcomes reported in the results section in three studies (Horiuchi 2009, Sheen 2008, Wilson-Sands 2015).

Was the study free from other risks of bias?

We considered conflicts of interest to be a potential source of bias. Three studies were supported by private sponsor grants (Bredesen 2016; Fordis 2005; Harris 2008), and one received support in terms of evaluation tool or e-learning modules development (Le 2010).

Overall risk of bias

Considering the risk of bias for allocation concealment, incomplete outcome data, and blinding of outcome assessors to be key domains we rated two trials as having a low risk of bias (Bredesen 2016, Perkins 2012), seven trials as having unclear risk of bias (Benjamin 2008, Hugenholtz 2008, Khatony 2009, Mäkinen 2006, Paladino 2007, Simonsen 2014, Wilson-Sands 2015) and the remaining seven trials as having high risk of bias (Fordis 2005, Harris 2008, Horiuchi 2009, Le 2010, Levine 2011, Maloney 2011, Sheen 2008).

Effects of interventions

See: **Summary of findings for the main comparison** Summary of findings: e-learning versus traditional learning for health professionals

The Summary of findings for the main comparison reports the effects of e-learning compared to traditional learning in terms of patient outcomes and health professionals' behaviours, skills and knowledge.

Primary outcomes

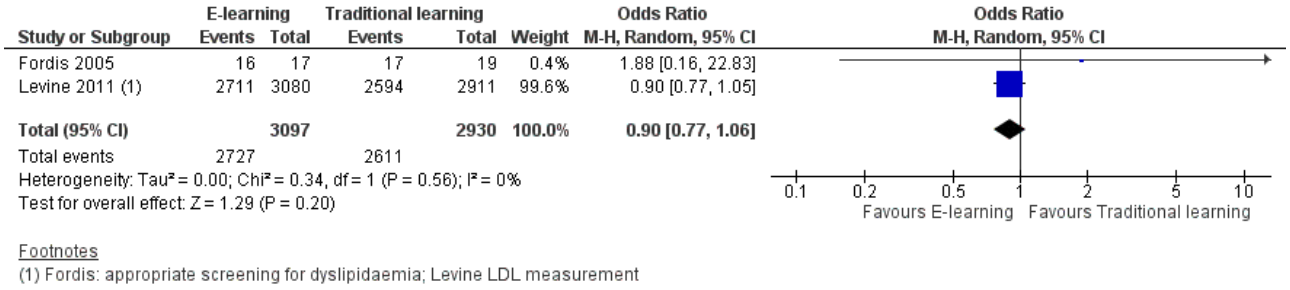
Patient outcomes

One study addressed patient outcomes (Levine 2011). This study randomised 168 primary care clinics (847 health professionals) to highly interactive e-learning versus face-to-face residential learning. After at least 12 months of exposure to the interventions, investigators used a patient administrative data review to compare the groups for two primary patient outcomes indicators. When compared with traditional learning, e-learning may make little or no difference in terms of the proportion of patients with target levels of low-density lipoprotein cholesterol (6399 patients; adjusted difference in improvement between the groups 4.0%, 95% CI −0.3 to 7.9) or the proportion of patients with target levels of glycated haemoglobin (3114 participants patients; adjusted difference in improvement between the groups 4.6%, 95% CI −1.5 to 9.8).

Health professionals' behaviours

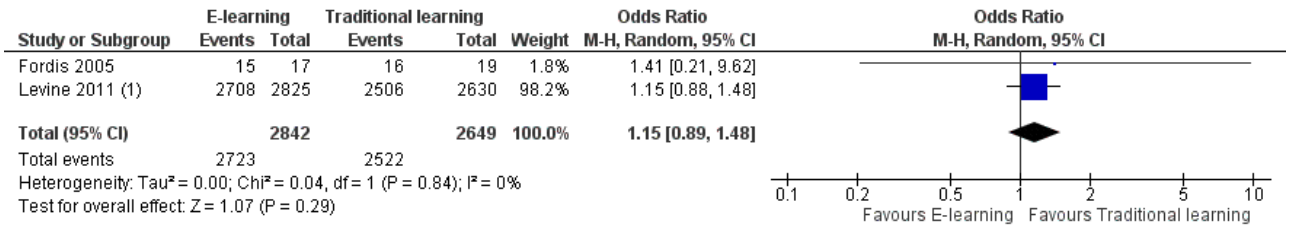
Two studies addressed this outcome in 950 health professionals (Fordis 2005; Levine 2011). Fordis 2005 randomised 103 primary care physicians to highly interactive and multifaceted e-learning versus face-to-face residential learning. After 12 weeks, investigators performed a patient chart review for 20 randomly selected doctors per group, comparing the groups in terms of appropriate screening for and treatment of dyslipidaemia. Levine 2011 reported data from three performance indicators, which we considered as behaviour outcomes: beta-blocker prescription, statin prescription, angiotensin-converting-enzyme (ACE) inhibitor or angiotensin-receptor antagonist prescription. In order to assess consistency, we explored all the possible combinations between the indicators reported by the two studies. When compared with traditional learning, e-learning may make little or no difference in terms of the proportion of patients appropriately screened or treated. In any combination of outcomes in meta-analysis, the resulting 95% CI always included both a beneficial and a harmful effect (Analysis 1.1, Figure 4; Analysis 1.2, Figure 5; Analysis 1.3; Analysis 1.4; Analysis 1.5). These results are from meta-analyses using random-effects models. The fixed-effect model yielded similar results (data not shown).

Figure 4. Forest plot of comparison: 1 Behaviours, outcome: 1.1 Patients appropriately screened (Fordis 2005 - screening for dyslipidaemia; Levine 2011 - LDL measurement).



Footnotes
(1) Fordis: appropriate screening for dyslipidaemia; Levine LDL measurement

Figure 5. Forest plot of comparison: 1 Behaviours, outcome: 1.2 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - statin prescription).



Footnotes
(1) Fordis appropriate treatment for dyslipidaemia; Levine statin prescription

Secondary outcomes

Health professionals' skills

It is uncertain whether e-learning improves or reduces health professionals' skills more than traditional learning, as we assessed the certainty of the evidence as very low: we included six trials in 2912 participants (0 to 12 weeks' follow-up) (Bredesen 2016; Mäkinen 2006; Perkins 2012; Sheen 2008; Simonsen 2014; Wilson-Sands 2015), but we could only pool data for two (Bredesen 2016; Simonsen 2014; Analysis 2.1; SMD 0.03, 95% CI −0.25 to 0.31, I^2 = 61%, 201 participants, 12 weeks' follow-up). We were unable to include the results from the largest trial, Perkins 2012, and two more trials (Mäkinen 2006, Sheen 2008), favouring traditional learning (2640 participants), or one trial favouring e-learning (Wilson-Sands 2015).

Perkins 2012 assessed performance in a cardiac arrest simulation test (CASTest). The full analysis on the mixed population of

participants showed little or no difference between the e-learning and the traditional learning group. However, the study authors provided us with unpublished data (Kimani 2015 [pers comm]) excluding students and participants with missing professional status from the analysis (2562 health professionals, 91% of all the professionals for skill outcomes). A separate analysis on the remaining participants showed that the proportion of health professionals passing the test was higher in the traditional learning group than the e-learning group (OR 1.46, 95% CI 1.22 to 1.76; Analysis 2.2).

Health professionals' knowledge

Eleven trials (3236 participants) assessed this outcome. Three trials in 154 participants reported the data poorly, precluding meta-analysis (Le 2010; Maloney 2011; Sheen 2008), but we could pool results from the remaining eight trials (3082 health professionals). Seven studies (3012 participants) assessed results immediately after the training intervention took place (Benjamin 2008; Fordis 2005; Harris 2008; Horiuchi 2009; Hugenholtz 2008; Khatony 2009; Paladino 2007; Perkins 2012). Three studies in 225 participants carried out the assessment 4 to 12 weeks after the training (Fordis 2005; Harris 2008; Horiuchi 2009): one of these studies assessed the outcome only after 4 weeks (Horiuchi 2009). For each study we used the longest follow-up data available.

E-learning may make little or no difference in health professionals' knowledge. We report results under both a fixed-effect model (SMD 0.04, 95% CI −0.03 to 0.11; Figure 6) and a random-effects model (SMD −0.09, 95% CI −0.27 to 0.09; Figure 7). The heterogeneity among the eight studies contributing to our meta-analyses was moderate (I^2 = 47%).

Figure 6. Forest plot of comparison: 3 Knowledge, outcome: 3.1 At any time (fixed-effect).

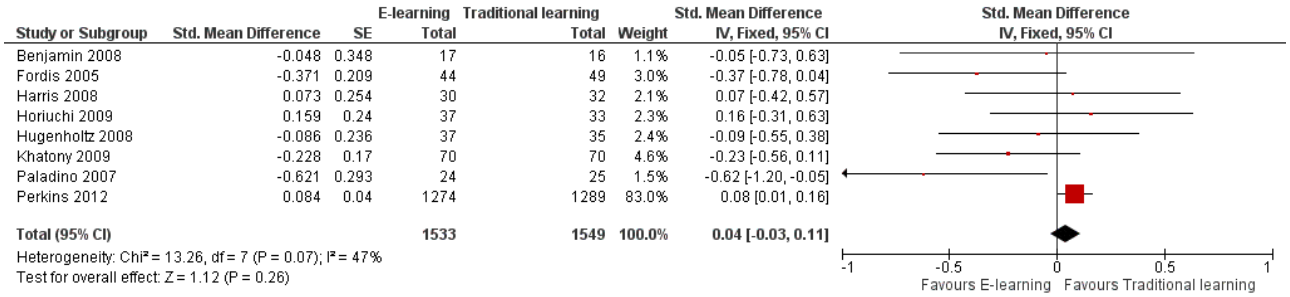
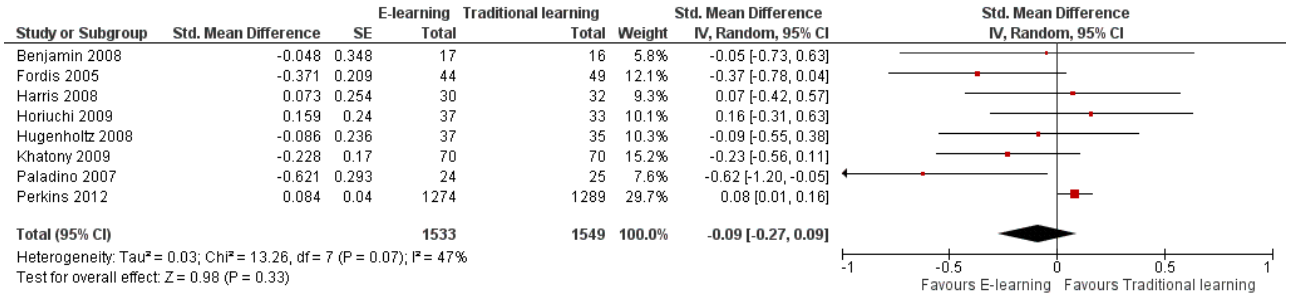


Figure 7. Forest plot of comparison: 3 Knowledge, outcome: 3.2 At any time (random-effects).



Separate analyses of studies with outcome measurement immediately after the training (Analysis 3.3) and after three or more months of follow-up (Analysis 3.4) provided similar results.

Assessment of reporting bias

We did not have enough data to perform reporting bias analyses.

Subgroup analysis and investigation of heterogeneity

Owing to paucity of data, we decided not to perform subgroup analyses.

Sensitivity analysis

Excluding studies assessed as being at overall high or unclear risk of bias was not applicable because we rated all the studies at high or unclear risk of bias except Perkins 2012; we did not identify any cross-over trials.

DISCUSSION

Summary of main results

This systematic review included 16 randomised studies: most of the these were small trials (only three trials involved more than 150 participants) at high or unclear risk of bias due to poor reporting. Our results suggest that compared to traditional learning, e-learning may lead to little or no difference in patient outcomes or health professionals' behaviours (low-certainty evidence), while the effect on health professionals' skills is unclear (very low-certainty evidence). E-learning may also make little or no difference compared to more traditional instructional methods on health professionals' knowledge (low-certainty evidence). In broad terms, e-learning is associated with no important benefits compared to traditional learning. The only large trial considered, at low risk of bias, favoured traditional learning for skills. However, readers

should interpret this noteworthy difference with great caution: our systematic review highlights how results of randomised trials were partially heterogenous, inconclusive and associated with negligible effect sizes.

Overall completeness and applicability of evidence

The randomised trials included in the review seemed to be sufficiently homogeneous in terms of included populations, comparison between e-learning versus traditional learning, and outcome measures. With the exception of one study involving childcare health consultants, all studies included doctors or nurses. However, reporting within the studies was often poor, with few details on educational content, systems and implementation factors. The description of the setting usually lacked information about how innovative e-learning was in the experimental context (e.g. early adoption, standard practice, etc.). In most cases it seems that e-learning was an innovative intervention being compared to the conventional approach.

Twelve trials compared an e-learning intervention with face-to-face learning, and two trials evaluated e-learning against guideline dissemination or availability. We believe these comparisons are relevant for many decisions on whether to choose one educational approach or another.

Certainty of evidence

Overall, we identified several methodological limitations during our assessment of risk of bias, prompting us to downgrade the certainty of evidence to low for all outcomes except health professionals' knowledge (Figure 2; Figure 3; Summary of findings for the main comparison). Incomplete outcome data was the dimension at highest risk of bias in terms of the number of studies assessed at high risk for this item. The number of participants who withdrew from or dropped out of the studies was more than 20% in five trials; in five more studies, authors did not state the percentage. The loss to follow-up may have introduced imbalances between the groups included in the analyses.

Potential biases in the review process

We identified several trials through our search strategy, but we did not search the grey literature or databases that might be relevant for some health professionals but do not primarily focus on randomised trials (e.g. CINAHL). We report differences between protocol and review below. We judge these differences as having no influence on the original objectives of this review, or not as potential sources of bias to our findings.

Agreements and disagreements with other studies or reviews

Previous systematic reviews have found e-learning to be associated with small positive effects compared with traditional educational interventions. In 2008, Cook and McDonald published a quantitative meta-analysis including 201 studies of Internet-based learning (Cook 2008a). The apparent discrepancy between our findings and their findings may be due to differences in the type of studies included: while we only considered randomised trials involving licensed health professionals, Cook 2008a also included non-randomised trials and studies with undergraduate participants. Just 2 of the 76 studies included in Cook's work had the same PICO framework of our review (Fordis 2005; Mäkinen 2006). Only 14% of participants in the studies they included were practicing health professionals (the other participants were all students).

A document from the US Department of Education reported the results of a review and meta-analysis of online learning studies for undergraduate students. They found that on average, the students in online learning environments performed modestly better than those receiving face-to-face instructions. We found little or no effect on learning outcomes, and one might speculate that e-learning tools fare better in younger populations. This phenomenon is well known in social sciences research as a 'cohort effect', defined as "the effect that having been born in a certain time, region, period or having experienced the same life experience (in the same time period) has on the development of a particular group" (Glen 2005).

AUTHORS' CONCLUSIONS

Implications for practice

Our results suggest in broad terms that e-learning does not itself result in major benefits for patient or health professional outcomes. Opting for traditional or e-learning approaches entails complex judgments, relating to the relative efficacy of the methods but also dimensions such as accessibility, usability, retention and costs. Traditional learning may be preferable in some instances, e.g. to improve knowledge or skills in small groups of health professionals when physical attendance is feasible, while e-learning programmes may be a better choice when the aim is to reach a large number of health professionals at a limited cost. Blended courses potentially balance the benefits of the two learning strategies.

The effectiveness of traditional learning means that e-learning is likely to have relatively similar effects, and powerful trials with prohibitively large sample sizes would be needed to show statistical superiority in some domain. Our results do not provide support for the superiority of e-learning. The results do not necessarily outweigh some benefits of e-learning, such as increased accessibility and flexibility. There is insufficient evidence to provide recommendations about accreditation, interactivity and length of e-learning programmes or about targeting of courses towards specific types of participants or contents. We have limited understanding of the characteristics that may influence the effectiveness of different e-learning programmes. Thus, our systematic review provides limited information to guide the choice or optimisation of components of e-learning interventions.

Implications for research

Although 16 randomised trials might seem a limited cohort, trials in education rarely benefit from commercial support, so the included evidence represents a valuable basis. Future trials might focus on additional core components of content, frequency of delivery, duration and intensity of e-learning, which might modify the effects of e-learning beyond those found in this review. There seems to be an opportunity for future trials to evaluate cost-effectiveness: everything being equal, costs and feasibility might represent the dimension where e-learning gains prominence.

Future studies should aim to use randomised designs with appropriate sample sizes, favouring the assessment of patient outcomes and health professionals' behaviours rather than skills or knowledge, and they should focus on the components of e-learning

that can eventually change behaviour as well as knowledge and skills.

Assessing outcomes at multiple time points during the study follow-up can determine the persistence of effects.

All studies, irrespective of the outcomes considered, should use predefined data scales and reporting rules in order to improve the account of the research questions under investigation.

More data are needed to evaluate the relative efficacy of e-learning in specific medical areas or rare conditions (i.e. e-learning programmes assisting in surgical teaching) and the importance of accreditation, interactivity and length of e-learning programmes.

The feasibility of these studies is challenged by the need for a large number of participants and long follow-up, but investigators may take existing educational settings providing training interventions into account as opportunities to override this problem. Finally, it may be more realistic to expect the development of studies that can inform practice using quasi-experimental designs, wait-list controls or stepped-wedged implementation.

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AV would like to dedicate this review to the memory of his brother Andrea, example of research in Economics and life.

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CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Benjamin 2008

Methods	**Study type**: randomised trial **Study arms**: 3
Participants	**Participants type**: childcare health consultants **Number randomised** (e-learning/control): 17/16 **Lost to follow-up**: not reported
Interventions	**E-learning type**: web training using photographs, quizzes and interactive multiple choice questions **E-learning interactivity**: high **E-learning blending**: alone **E-learning duration**: short; completion within 3 weeks (mean time spent on training 120 minutes) **Control type**: face-to-face training **Control duration**: 3 hours **Follow-up** (from the end of the intervention to the last outcome assessment): short - 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: not stated **Setting**: community setting
Outcomes	**Primary**: knowledge (by an non-validated test) **Secondary**: time spent on training **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: August 2005-June 2006 **Funding source**: Centers for Disease Control and Prevention (CDC), North Carolina Division of Public Health, Child Care Bureau **Declaration of interest**: none declared

Benjamin 2008 *(Continued)*

	Country: USA
	Topic: childhood overweight management

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported
Was the allocation adequately concealed?	Low risk	Sealed envelopes with a randomisation sequence developed by the study biostatistician
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	Unclear risk	No information reported
Were incomplete outcome data adequately addressed?	Unclear risk	No information reported
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: low Risk of attrition bias: unclear Risk of detection bias: unclear

Bredesen 2016

Methods	**Study type**: randomised trial
	Study arms: 2
Participants	**Participants type**: nurses
	Number randomised (e-learning/control): 23/21

Bredesen 2016 *(Continued)*

	Lost to follow-up (number(%); (e-learning/control)): 13(56.5%)/13(61.9%)
Interventions	**E-learning type**: patient cases, photos and schematic illustration **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: not reported **Control type**: traditional classroom lecture **Control duration**: 45 minutes **Follow-up** (time from the end of the intervention to the last outcome assessment): 0 weeks (immediately after) and three months later **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: skills **Secondary**: none **Times the outcomes were assessed after the intervention**: 2
Notes	**Study dates**: May 2012-December 2012 **Funding source**: Oslo University Hospital, Norwegian Nurses Organisation, University of Oslo and Sophies Minde Ortopedi AS **Declaration of interest**: no competing interest **Country**: Norway **Topic**: pressure ulcer risk assessment and classification **Other**: authors provided unpublished data regarding pressure ulcer classification (Brendsen 2016 [pers comm])

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Envelope shuffling
Was the allocation adequately concealed?	Low risk	Envelope shuffling
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Low risk	Chi^2/Fisher's Exact test not significant between the 2 groups

Bredesen 2016 *(Continued)*

Were incomplete outcome data adequately addressed?	Low risk	No incomplete data at post-test immediately after the training
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	Outcome is not likely to be influenced by lack of blinding in this study
Was the study adequately protected against contamination?	Unclear risk	Contamination is unlikely
Was the study free from selective outcome reporting?	Low risk	The published report includes all expected outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	High risk	Private sponsor Sophies Minde Ortopedi AS
OVERALL RISK OF BIAS	Low risk	Risk of selection bias: low Risk of attrition bias: low Risk of detection bias: low

Fordis 2005

Methods	**Study type**: randomised trial **Study arms**: 3
Participants	**Participants type**: primary care physicians **Number randomised** (e-learning/control): 52/51 **Lost to follow-up** (number(%); (e-learning/control)): 8(15.4%)/2(3.9%)
Interventions	**E-learning type**: online lecture, interactive cases with feedback, enabling tools, supporting resources, access to expert advice **E-learning interactivity**: high **E-learning blending**: core and essential **E-learning duration**: short - at participants convenience during a 2-week period (mean time spent on training 1.4 hours for 3 session) **Control type**: live lecture interactive cases with feedback, enabling tools, supporting resources, access to expert advice **Control duration**: 1.5-2 hours **Follow-up** (time from the end of the intervention to the last outcome assessment): 12 weeks **CanMEDS framework area**: medical expertise **Regulation**: formally accredited

Fordis 2005 *(Continued)*

	Setting: primary care
Outcomes	**Primary**: knowledge (by a validated test), behaviours (appropriate screening and treatment for dyslipidaemia) **Secondary**: time spent on training, satisfaction **Times the outcomes were assessed after the intervention**: 2
Notes	**Study dates**: August 2001-July 2002 **Funding source**: AstraZeneca Pharmaceuticals **Declaration of interest**: grant support from AstraZeneca and other pharmaceutical companies **Country**: USA **Topic**: cholesterol management **Other**: authors provided single participants data about knowledge as requested (Jason 2015 [pers comm])

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Random number generator
Was the allocation adequately concealed?	Low risk	Centralised randomisation scheme
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	High risk	Major imbalance in missing data between groups: 15.4% in the e-learning group and 5.8% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	Data analyst blinded to the identification of participants
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	High risk	Study supported by a grant from AstraZeneca Pharmaceuticals.

Fordis 2005 *(Continued)*

OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias: low

Harris 2008

Methods	**Study type**: randomised trial **Study arms**: 3
Participants	**Participants type**: primary care physicians **Number randomised** (e-learning/control): 49/50 **Lost to follow-up** (number(%); (e-learning/control)): 19(38.8%)/18(36.0%)
Interventions	**E-learning type**: on-line lectures **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: short - 4 hours **Control type**: live lecture **Control duration**: 4 hours **Follow-up** (time from the end of the intervention to the last outcome assessment): long - 12 weeks **CanMEDS framework area**: medical expertise **Regulation**: formally accredited **Setting**: primary care
Outcomes	**Primary**: knowledge (by a validated test) **Secondary**: time spent on training, satisfaction **Times the outcomes were assessed after the intervention**: 2
Notes	**Study dates**: September 2005 **Funding source**: Small Business Innovation and Research (SBIR) grant **Declaration of interest**: none declared **Country**: USA **Topic**: chronic pain **Other**: we decided to include this study after discussion about the outcome measure used. The know pain 50 assesses a mix of knowledge, attitudes and beliefs but at the end we considered that the most of the items regard knowledge.

Risk of bias

Bias	Authors' judgement	Support for judgement

Harris 2008 *(Continued)*

Was the allocation sequence adequately generated?	Low risk	Blind name draw
Was the allocation adequately concealed?	Low risk	Centralised randomisation scheme
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Unclear risk	No information reported
Were incomplete outcome data adequately addressed?	High risk	Missing data 38.8% in the e-learning group and 36.0% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Low risk	The authors controlled the participants' room change
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	High risk	The development of the online CME programme and the research study were supported by Small Business Innovation and Research (SBIR) grants
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias: unclear

Horiuchi 2009

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses **Number randomised** (e-learning/control): 45/48 **Lost to follow-up** (number(%); (e-learning/control)): 8(17.8%)/15(31.2%)
Interventions	**E-learning type**: four 30-minute online classes **E-learning interactivity**: low **E-learning bending**: alone

Horiuchi 2009 *(Continued)*

	E-learning duration: short - 120 minutes **Control type**: four 90-minute evening lectures **Control duration**: 360 minutes **Follow-up** (time from the end of the intervention to the last outcome assessment): long - 4 weeks **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: knowledge (by an non-validated test) **Secondary**: satisfaction **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: August 2005-November 2006 **Funding source**: Japanese Ministry of Education Scientific Research Grant **Declaration of interest**: none declared **Country**: Japan **Topic**: evidence-based medicine

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Computerised random number generator
Was the allocation adequately concealed?	Low risk	Centralised randomisation scheme and sealed opaque envelopes
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	High risk	Several imbalance between group in the demographics of participants
Were incomplete outcome data adequately addressed?	High risk	Major imbalance in missing data between groups: 17.8% in the e-learning group and 31.2% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Unclear risk	No information reported

Horiuchi 2009 *(Continued)*

Was the study free from selective outcome reporting?	High risk	Inconsistencies between outcomes declared in the Methods and outcomes reported in the Results
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias: unclear

Hugenholtz 2008

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: occupational physicians **Number randomised** (e-learning/control): 37/35 **Lost to follow-up** (number(%); (e-learning/control)): 0/2(5.4%)
Interventions	**E-learning type**: individual e-learning **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: short - 30 minutes **Control type**: live lecture **Control duration**: 30 minutes **Follow-up** (time from the end of the intervention to the last outcome assessment): short - 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: formally accredited **Setting**: occupational medicine
Outcomes	**Primary**: knowledge (by an non-validated test) **Secondary**: none **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: December 2006 **Funding source**: none declared **Declaration of interest**: none declared **Country**: Netherlands

Hugenholtz 2008 *(Continued)*

Topic: Mental health

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported
Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	Unclear risk	No information reported
Were incomplete outcome data adequately addressed?	Low risk	The proportion of missing data was unlikely to overturn the study result: 0% in the e-learning group and 5.4% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Low risk	It is unlikely that communication between intervention and control groups could have occurred
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: low Risk of detection bias: unclear

Khatony 2009

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses **Number randomised** (e-learning/control): 70/70 **Lost to follow-up**: not reported

Khatony 2009 *(Continued)*

Interventions	**E-learning type**: 1 week educational material access, chat room, emailing and telephone availability for answering questions **E-learning interactivity**: high **E-learning blending**: alone **E-learning duration**: long - 1 week **Control type**: face-to-face interactive lecture **Control duration**: 3 hours **Follow-up** (time from the end of the intervention to the last outcome assessment): short - 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: knowledge (by a validated test) **Secondary**: none **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: winter 2007 **Funding source**: none declared **Declaration of interest**: no competing interest declared **Country**: Iran **Topic**: AIDS

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported
Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	Unclear risk	No information reported
Was knowledge of the allocated interventions ade-	Unclear risk	No information reported

Khatony 2009 *(Continued)*

quately prevented during the study?		
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: unclear Risk of detection bias: unclear

Le 2010

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: paediatricians **Number randomised** (e-learning/control): 15/9 **Lost to follow-up** (number(%); (e-learning/control)): 4(26.7%)/0(0%)
Interventions	**E-learning type**: 2 teleconferences, access to a website with 6 interactive multimedia earning modules and a CD-ROM with the same learning modules **E-learning interactivity**: high **E-learning blending**: core and essential **E-learning duration**: long - 6 weeks to complete the modules **Control type**: guidelines dissemination - authors reply on 15 July 2015 (Cabana 2015 [pers comm]) **Control duration**: 0 weeks **Follow-up** (time from the end of the intervention to the last outcome assessment): 32 weeks **CanMEDS framework area**: medical expertise **Regulation**: formally accredited **Setting**: primary care
Outcomes	**Primary**: satisfaction **Secondary**: knowledge (by an non-validated test), attitudes, self-reported prescription, self-reported guidelines familiarity **Times the outcomes were assessed after the intervention**: 2

Le 2010 *(Continued)*

Notes	**Study dates**: February 2007-March 2008 **Funding source**: none declared **Declaration of interest**: no competing interest declared **Country**: USA **Topic**: asthma

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	High risk	Authors matched participants from the same practice into pairs: within each pair, they randomised one participant to the control group and the other to the intervention group
Was the allocation adequately concealed?	Low risk	Unit of allocation was by institution, team or professional and allocation performed on all units at the start of the study
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	High risk	Some imbalance between group in the demographics of participants
Were incomplete outcome data adequately addressed?	High risk	Major imbalance in missing data between groups: 26.3% in the e-learning group and 0.0% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Unclear risk	Participants were allocated within a practice and it is possible that communication between intervention and control professionals could have occurred
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	High risk	Indegene Inc gave assistance in developing the learning modules
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias: unclear

Levine 2011

Methods	**Study type**: cluster-randomised trial

Levine 2011 *(Continued)*

	Study arms: 2
Participants	**Participants type**: healthcare providers (not otherwise specified) **Number randomised** (e-learning/control): 84 clinics (385 providers, 4024 patients)/84 clinics (462 providers, 3727 patients) **Lost to follow-up** (number(%); (e-learning/control)): 180 providers (47%), 944 patients (24.5%)/266 providers (57%), 816 patience (22%)
Interventions	**E-learning type**: multicomponent website (relevant clinical guidelines, monthly summaries of pertinent peer-review manuscripts, downloadable practice tools and patient educational materials) and pushed email cues with educational content **E-learning interactivity**: high **E-learning blending**: core and essential **E-learning duration**: long - 108 weeks **Control type**: clinical guidelines website and the medical letter subscription **Control duration**: 108 weeks **Follow-up** (time from the end of the intervention to the last outcome assessment): 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: formally accredited **Setting**: primary care
Outcomes	**Primary**: 7 clinical indicators of performance improvement (5 of health professionals' behaviour, 2 of patient outcomes) **Secondary**: composite clinical indicator score **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: January 2002-December 2008 **Funding source**: Veterans Affairs Health Services Research and Development Grant **Declaration of interest**: none declared **Country**: USA **Topic**: care after myocardial infarction

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported
Was the allocation adequately concealed?	Low risk	Unit of allocation was by team or professional and allocation performed on all units at the start of the study
Were baseline outcome measurements similar?	Low risk	No important differences across study groups

Levine 2011 *(Continued)*

Were baseline characteristics similar?	High risk	Several imbalances between group in several participation measures (participants' providers, website visits, etc)
Were incomplete outcome data adequately addressed?	High risk	Missing patient data: 24.5% in the e-learning group and 22.0% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Low risk	Allocation by clinics
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias: unclear

Maloney 2011

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses, physiotherapists, others health professionals **Number randomised** (e-learning/control): 67/68 **Lost to follow-up** (number(%); (e-learning/control)): 24(36%)/19(28%)
Interventions	**E-learning type**: web-based discussions available even by phone, DVD comprising the multimedia used in the web-based programme, self-directed reading and formative quizzes to interactive skills-practice sessions with feedback opportunities **E-learning interactivity**: high **E-learning blending**: core and essential **E-learning duration**: short - 7 hours **Control type**: face-to-face intervention; copy of the presentation slides, reference to further readings, and a DVD of the assessment procedures to be covered in the seminar **Control duration**: 7 hours **Follow-up** (time from the end of the intervention to the last outcome assessment): 1 week

Maloney 2011 *(Continued)*

	CanMEDS framework area: medical expertise **Regulation**: not specified **Setting**: rehabilitation
Outcomes	**Primary**: knowledge (by an non-validated test) **Secondary**: satisfaction, self-reported change in practice **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: not reported **Funding source**: Department of Health, Victoria, Australia **Declaration of interest**: none declared **Country**: Australia **Topic**: falls prevention exercise

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Computerised random number sequence
Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	High risk	Missing patients data 35.8% in the e-learning group and 27.9% in the control group
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	Blinded outcome assessment
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low

Maloney 2011 *(Continued)*

	Risk of attrition bias: high
	Risk of detection bias: low

Mäkinen 2006

Methods	**Study type**: randomised trial
	Study arms: 3
Participants	**Participants type**: nurses
	Number randomised (e-learning/control): 20/16
	Lost to follow-up: not reported
Interventions	**E-learning type**: multimedia (video clips and pictures), a short written explanation of the multimedia, links to the databases extending the amount of information if needed and questions between the content pages with correct answers presented
	E-learning interactivity: high
	E-learning blending: alone
	E-learning duration: short - 15-30 minutes
	Control type: a certified trainer gave a 4-h basic life support and defibrillation course
	Control duration: 240 minutes
	Follow-up (time from the end of the intervention to the last outcome assessment): 2 weeks
	CanMEDS framework area: medical expertise
	Regulation: not specified
	Setting: secondary (hospital) care
Outcomes	**Primary**: skills (OSCE)
	Secondary: none
	Times the outcomes were assessed after the intervention: 1
Notes	**Study dates**: not reported
	Funding source: none declared
	Declaration of interest: none declared
	Country: Finland
	Topic: basic life support

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported

Mäkinen 2006 *(Continued)*

Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Unclear risk	No information reported
Were incomplete outcome data adequately addressed?	Unclear risk	No information reported
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	Observers blinded to the educational method of the groups
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: unclear Risk of detection bias: low

Paladino 2007

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses **Number randomised** (e-learning/control): 25/24 **Lost to follow-up**: not reported
Interventions	**E-learning type**: e-learning training by PowerPoint **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: short - 40 minutes **Control type**: on-site training by PowerPoint **Control duration**: 120 minutes

Paladino 2007 *(Continued)*

	Follow-up (time from the end of the intervention to the last outcome assessment): short - 0 weeks (immediately after) **CanMEDS framework area**: management **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: knowledge (by an non-validated test) **Secondary**: none **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: not reported **Funding source**: none declared **Declaration of interest**: none declared **Country**: Brazil **Topic**: quality tools

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Unclear risk	No information reported
Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Unclear risk	No information reported
Were incomplete outcome data adequately addressed?	Unclear risk	No information reported
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes

Paladino 2007 *(Continued)*

Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: unclear Risk of detection bias: unclear

Perkins 2012

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: physicians, nurses, students **Number randomised** (e-learning/control): 1843/1889 (1255 vs 1271 without students) **Lost to follow-up** (number(%); (e-learning/control)): 476(25.8%)/523(27.7%)
Interventions	**E-learning type**: 4 e-lectures and 6 interactive workshops **E-learning interactivity**: high **E-learning blending**: core and essential **E-learning duration**: 2 days (short) **Control type**: conventional advanced life support **Control duration**: 2 days **Follow-up** (time from the end of the intervention to the last outcome assessment): 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: pre-hospital care (cardiopulmonary resuscitation)
Outcomes	**Primary**: skills **Secondary**: knowledge (by a validated test) **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: December 2008-October 2010 **Funding source**: Resuscitation Council (UK) **Declaration of interest**: declared on www.apconline.org **Country**: UK, Australia **Topic**: advanced life support **Other**: authors provided unpublished data (Kimani 2015 [pers comm])
Risk of bias	

Perkins 2012 *(Continued)*

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Electronic randomisation
Was the allocation adequately concealed?	Low risk	Centralised randomisation scheme
Were baseline outcome measurements similar?	Low risk	Knowledge pre-course test better in e-learning group. Since the final difference in knowledge is in the opposite direction (favouring traditional learning), there is no indication of a bias.
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	Low risk	The proportion of missing data was unlikely to overturn the study results; the study results were analysed on an intention-to-treat basis
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	The authors were unable to ensure blinding of outcome assessment. However we judged that the outcome measurement was not likely to be influenced by lack of blinding, as the process of measurement was structured.
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Low risk	Risk of selection bias: low Risk of attrition bias: low Risk of detection bias: unclear (the blinding of outcome assessors is not explicitly stated) Considering the low risk of bias across most dimensions, we considered the study to be at an overall minimal risk of bias

Sheen 2008

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses **Number randomised** (e-learning/control): 22/20

Sheen 2008 *(Continued)*

	Lost to follow-up: not reported
Interventions	**E-learning type**: audio, video and PowerPoint presentation format **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: short - 5.5 hours **Control type**: traditional in class programme **Control duration**: not reported **Follow-up** (time from the end of the intervention to the last outcome assessment): short - 0 weeks, (immediately after) **CanMEDS framework area**: medical expertise, communication, management, scholar **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: knowledge (by an non-validated test) and skills in several professional dimensions **Secondary**: satisfaction **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: 2004-2005 **Funding source**: Taiwan National Science Council **Declaration of interest**: none declared **Country**: Taiwan **Topic**: nursing care

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Randomisation by coin flip
Was the allocation adequately concealed?	Low risk	Randomisation by coin flip
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	High risk	Participants who did not complete the courses were excluded and not used in data analysis
Was knowledge of the allocated interventions ade-	High risk	Neither participants nor evaluators were blinded

Sheen 2008 *(Continued)*

quately prevented during the study?		
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	High risk	No result provided
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	High risk	Risk of selection bias: low Risk of attrition bias: high Risk of detection bias:high

Simonsen 2014

Methods	**Study type**: randomised trial **Study arms**: 2
Participants	**Participants type**: nurses **Number randomised** (e-learning/control): 92/91 **Lost to follow-up** (number(%); (e-learning/control)): 17(18.5%)/9(9.9%)
Interventions	**E-learning type**: interactive online tests, hints and suggested solutions; access to a collection of tests with feedback on answers and a printout of the compendium **E-learning interactivity**: high **E-learning blending**: alone **E-learning duration**: short - 2 days **Control type**: conventional classroom and self-study **Control duration**: 2 days **Follow-up** (time from the end of the intervention to the last outcome assessment): 2-4 weeks **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: secondary (hospital) care
Outcomes	**Primary**: skills **Secondary**: certainty **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: September 2007-April 2009

Simonsen 2014 *(Continued)*

Funding source: South-East Norway Health Authorities and Innlandet Hospital Trust

Declaration of interest: commercial interest for one the authors

Country: Norway

Topic: drug dose calculation

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Predefined computer-generated lists
Was the allocation adequately concealed?	Unclear risk	No information reported
Were baseline outcome measurements similar?	Low risk	No important differences across study groups
Were baseline characteristics similar?	Low risk	No important differences across study groups
Were incomplete outcome data adequately addressed?	Low risk	Imbalance in missing data between groups: 18.5% in the e-learning group and 9.9% in the control group but the proportion of missing data was unlikely to overturn the study results and the study results were analysed on an intention-to-treat basis
Was knowledge of the allocated interventions adequately prevented during the study?	Unclear risk	No information reported
Was the study adequately protected against contamination?	Unclear risk	No information reported
Was the study free from selective outcome reporting?	Low risk	No evidence of selective reporting of outcomes
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: low Risk of detection bias: unclear

Wilson-Sands 2015

Methods	**Study type**: randomised trial

Wilson-Sands 2015 *(Continued)*

	Study arms: 2
Participants	**Participants type**: mixed health professionals **Number randomised** (e-learning/control): 25/20 **Lost to follow-up**: not reported
Interventions	**E-learning type**: online interactive patient care scenarios **E-learning interactivity**: low **E-learning blending**: alone **E-learning duration**: not reported **Control type**: instructor led training **Control duration**: not reported **Follow-up** (time from the end of the intervention to the last outcome assessment): 0 weeks (immediately after) **CanMEDS framework area**: medical expertise **Regulation**: not specified **Setting**: pre-hospital care (cardiopulmonary resuscitation)
Outcomes	**Primary**: skills (3 outcome: correct compressions, correct ventilations, correct CPR cycles) **Secondary**: none **Times the outcomes were assessed after the intervention**: 1
Notes	**Study dates**: not reported **Funding source**: not reported **Declaration of interest**: not reported **Country**: USA **Topic**: Basic Life Support

Risk of bias

Bias	Authors' judgement	Support for judgement
Was the allocation sequence adequately generated?	Low risk	Cards shuffling
Was the allocation adequately concealed?	Unclear risk	Cards shuffling
Were baseline outcome measurements similar?	Unclear risk	No information reported
Were baseline characteristics similar?	Unclear risk	Unclear differences across study groups

Wilson-Sands 2015 *(Continued)*

Were incomplete outcome data adequately addressed?	Unclear risk	No information reported
Was knowledge of the allocated interventions adequately prevented during the study?	Low risk	Outcome is not likely to be influenced by lack of blinding in this study
Was the study adequately protected against contamination?	Unclear risk	Contamination is unlikely
Was the study free from selective outcome reporting?	High risk	The results of a written exam is not reported
Was the study free from other risks of bias (e.g. conflicts of interest)?	Low risk	No evidence of other risk of bias
OVERALL RISK OF BIAS	Unclear risk	Risk of selection bias: unclear Risk of attrition bias: unclear Risk of detection bias: low

CME: continuing medical education; **OSCE**: objective structured clinical examination.

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Alfieri 2012	Not complying with participants inclusion criteria (residents)
Allison 2005	Not complying with control inclusion criteria (e-learning as a control)
Anderson 2006	Not complying with study type inclusion criteria (no randomisation)
Andolsek 2013	Not complying with study type inclusion criteria (no randomisation)
Bayar 2009	Not complying with control inclusion criteria (no intervention)
Beckley 2000	Not complying with intervention inclusion criteria (not delivered by Internet)
Beeckman 2008	Not complying with participants inclusion criteria (residents)
Bello 2005	Not complying with participants inclusion criteria (residents)
Benedict 2013	Not complying with participants inclusion criteria (students)
Beyea 2008	Not complying with participants inclusion criteria (residents)
Bode 2012	Not complying with participants inclusion criteria (trainees)
Boespflug 2015	Not complying with study type inclusion criteria (no randomisation)

Study	Reason for exclusion
Bonevski 1999	Not complying with intervention inclusion criteria (computerised feedback system)
Browne 2004	Not complying with participants inclusion criteria (trainees)
Buijze 2012	Not complying with control inclusion criteria (no intervention)
Butler 2012	Not complying with control inclusion criteria (no intervention)
Butzlaff 2004	Not complying with control inclusion criteria (no intervention)
Carney 2011	Not complying with control inclusion criteria (no intervention)
Carney 2012	Not complying with control inclusion criteria (no intervention)
Casap 2011	Not complying with study type inclusion criteria (no randomisation)
Chan 1999	Not complying with control inclusion criteria (e-learning as a control)
Chenkin 2008	Not complying with participants inclusion criteria (mixed residents and staff physicians). No answer from the authors to request of separated data (on 5 July 2015)
Chung 2004	Not complying with intervention inclusion criteria (e-learning programmes on bio-terrorism; focusing on non-clinical medical topics defined as subjects different from the CanMEDS 7 physicians roles; mixed residents and staff physicians)
Cook 2008	Not complying with participants inclusion criteria (residents)
Crenshaw 2010	Not complying with intervention inclusion criteria (computerised feedback system)
Curtis 2007	Not complying with intervention inclusion criteria (e-learning not core and essential: audit and feedback in the intervention but not in the control arm)
De Beurs 2015	Not complying with outcome inclusion criteria (self-reported knowledge)
De Beurs 2016	Not complying with control inclusion criteria (e-learning and usual approach vs usual approach alone)
Dimeff 2011	Not complying with control inclusion criteria (e-learning as a control)
Esche 2015	Not providing data about health professionals randomised to the intervention/control groups. Authors stated their inability to provide us you with the requested information (Esche 2015 [pers comm])
Estrada 2010	Not complying with intervention inclusion criteria (e-learning not core and essential)
Estrada 2011	Not complying with intervention inclusion criteria (e-learning not core and essential)
Fary 2015	Not complying with control inclusion criteria (no intervention)
Fisher 2014	Not complying with control inclusion criteria (no intervention)
Foroudi 2013	Not complying with control inclusion criteria (e-learning as a control)
Fox 2001	Not complying with control inclusion criteria (e-learning as a control)

Study	Reason for exclusion
Franchi 2016	Not complying with control inclusion criteria (e-learning in both the arms)
Funk 2010	Not complying with study type inclusion criteria (discussion about PULSE trial). No answer from the authors to request of data (on 5 July 2015)
Gerbert 2002	Not complying with control inclusion criteria (no intervention). No answer from the authors to our request of explanation about control intervention (on 12 April 2015)
Ghoncheh 2014	Not complying with study type inclusion criteria (protocol). No answer from the authors to request of data (on 12 April 2015)
Gordon 2011a	Not complying with control inclusion criteria (no intervention)
Gordon 2011b	Not complying with participants inclusion criteria (trainees)
Gordon 2013a	Not complying with participants inclusion criteria (trainees)
Gordon 2013b	Not complying with study type inclusion criteria (review)
Granpeesheh 2010	Not complying with participants inclusion criteria (trainees)
Gyorki 2013	Not complying with participants inclusion criteria (residents)
Hansen 2007	Not complying with study type inclusion criteria (no randomisation)
Harris 2013	Not complying with control inclusion criteria (no intervention)
Hearty 2013	Not complying with participants inclusion criteria (residents)
Houwink 2014	Not complying with control inclusion criteria (no intervention)
Jensen 2009	Not complying with control inclusion criteria (no intervention)
Kemper 2002	Not complying with control inclusion criteria (no intervention) (Kemper 2015 [pers comm])
Kerfoot 2010	Not complying with control inclusion criteria (no intervention)
Kerfoot 2012	Not complying with control inclusion criteria (e-learning as a control)
Khanal 2014	Not complying with intervention inclusion criteria (the intervention was not distributed by the Internet)
Kim 2014	Not complying with control inclusion criteria (no intervention)
Kobak 2005	Not complying with participants inclusion criteria (mixed residents and staff physicians). No answer from the authors to request of separated data (on 2 July 2015)
Kontio 2011	Not complying with control inclusion criteria (same intervention as in the e-learning group) (Kontio 2015 [pers comm])
Kontio 2013	Not complying with control inclusion criteria (same intervention as in the e-learning group) (Kontio 2015 [pers comm])
Kontio 2014	Not complying with control inclusion criteria (same intervention as in the e-learning group) – as in the authors email received on 17 August 2015

Study	Reason for exclusion
Legris 2011	Not complying with control inclusion criteria (no intervention) (Lalonde 2015 [pers comm])
Liaw 2015	Not complying with control inclusion criteria (no intervention) (Liaw 2016 [pers comm])
Little 2013	Not complying with control inclusion criteria (no intervention)
Liu 2014a	Not complying with control inclusion criteria (no intervention)
Liu 2014b	Not complying with control inclusion criteria (no intervention)
Lu 2009	Not complying with participants inclusion criteria (students)
Maloney 2012	Not complying with study type inclusion criteria (economic analysis)
Markova 2013	Not complying with control inclusion criteria (e-learning intervention)
Marshall 2014	Not complying with outcome inclusion criteria (satisfaction)
McCormack 2012	Not complying with participants inclusion criteria (students)
McCrow 2014	Not complying with control inclusion criteria (no intervention)
Meckfessel 2011	Not complying with participants inclusion criteria (students)
Midmer 2006	Not complying with control inclusion criteria (no intervention). No answer from the authors to request of data (on 31 May 2015)
Moja 2008	Not complying with study type inclusion criteria (protocol). Data still not available (answer from the authors to request of data on 09 January 2018)
Moorthy 2003	Not complying with participants inclusion criteria for participants (trainees)
Moreira 2015	Not complying with control inclusion criteria (no intervention)
NCT00394017	Not complying with control inclusion criteria (no intervention)
NCT00815724	Not complying with control inclusion criteria (no intervention)
NCT00934141	Not complying with participants inclusion criteria (patients)
NCT00962455	Not complying with control inclusion criteria (no intervention)
NCT01326936	Not complying with participants inclusion criteria (trainees)
NCT01427660	Not complying with participants inclusion criteria (community health workers[a])
NCT01834521	Not complying with participants inclusion criteria (patients)
NCT01955005	Not complying with participants inclusion criteria (patients)
Nesterowicz 2015	Not complying with study type inclusion criteria (no randomisation)
Paul 2013	Not complying with study type inclusion criteria (protocol) and with control inclusion criteria (no intervention)

Study	Reason for exclusion
Pearce-Smith 2005	Not complying with participants inclusion criteria (mixed clinicians and managers). No answer from the authors to request of separated data (on 25 July 2015)
Pelayo-Alvarez 2011	Not complying with control inclusion criteria (no specific training was organised for the control group) (Pelayo-Alvarez 2015 [pers comm])
Perkins 2010	Not complying with intervention inclusion criteria (intervention provided by audio recording)
Pham 2013	Not complying with control inclusion criteria (no intervention)
Pham 2016	Not complying with control inclusion criteria (no control group) (Pham 2016 [pers comm])
Platz 2010	Not complying with control inclusion criteria (no intervention)
Rafalski 2004	Not complying with study type inclusion criteria (no randomisation)
Rankin 2013	Not complying with control inclusion criteria (e-learning group as control group): although the on-line tutorial was mandatory just for intervention group participants, all but 2 (out of 67) participants in the control group chose to do the tutorial.
Ruzek 2012	Not complying with study type inclusion criteria (protocol). No answer from the authors to request of data (on 12 April 2015)
Schermer 2011	Not complying with study type inclusion criteria (no randomisation)
Schopf 2012	Not complying with control inclusion criteria (no intervention as a control in the first part and e-learning vs e-learning in the second part)
Sharma 2013	Not complying with participants inclusion criteria for participants (trainees)
Shaw 2011	Not complying with outcomes inclusion criteria (self-reported outcomes)
Simpson 2009	Not complying with study type inclusion criteria (protocol) and with control inclusion criteria (no intervention)
Smeekens 2011	Not complying with control inclusion criteria (no intervention)
Soh 2010	Not complying with participants inclusion criteria (students)
Stein 2015	Not complying with outcome inclusion criteria (patient-reported outcome)
Stewart 2005	Not complying with control inclusion criteria (no intervention)
Sung 2008	Not complying with study type inclusion criteria (no randomisation)
Thompson 2012	Not complying with participants inclusion criteria (trainees)
Tung 2014	Not complying with study type inclusion criteria (no randomisation)
Valish 1975	Not complying with intervention inclusion criteria (not delivered by Internet)
Van de Steeg 2012	Not complying with study type inclusion criteria (protocol) and with control inclusion criteria (no intervention)

Study	Reason for exclusion
Van Stiphout 2015	Not complying with control inclusion criteria (e-learning and usual approach vs usual approach alone)
Veredas 2014	Not complying with participants inclusion criteria (students)
Vidal-Pardo 2013	Not complying with control inclusion criteria (no intervention)
Viguier 2015	Not complying with control inclusion criteria (no intervention)
Wakefield 2014	Not complying with control inclusion criteria (no intervention)
Ward 2005	Not complying with study type inclusion criteria (protocol). No answer from the authors to our request of data (on 28 June 2015, email)
Weaver 2012	Not complying with control inclusion criteria (e-learning as a control)
Wehrs 2007	Not complying with study type inclusion criteria (no randomisation)
Weston 2008	Not complying with control inclusion criteria (no intervention on the same topic)
Worm 2013	Not complying with participants inclusion criteria (trainees)
Yao 2015	Not complying with control inclusion criteria (no intervention)

[a]Community health workers (CHW) are members of a community who are chosen by community members or organisations to provide basic health and medical care to their community.

Characteristics of studies awaiting assessment *[ordered by study ID]*

Vollmar 2010

Methods	
Participants	
Interventions	
Outcomes	
Notes	Not yet assessed

DATA AND ANALYSES

Comparison 1. Behaviours

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Patients appropriately screened (Fordis 2005 - screening for dyslipidaemia; Levine 2011 - LDL measurement)	2	6027	Odds Ratio (M-H, Random, 95% CI)	0.90 [0.77, 1.06]
2 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - statin prescription)	2	5491	Odds Ratio (M-H, Random, 95% CI)	1.15 [0.89, 1.48]
3 Patients appropriately screened (Fordis 2005 - screening for dyslipidaemia; Levine 2011 - HbA1c measurement)	2	3056	Odds Ratio (M-H, Random, 95% CI)	0.85 [0.69, 1.06]
4 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - beta-blocker prescription)	2	6027	Odds Ratio (M-H, Random, 95% CI)	1.12 [0.97, 1.29]
5 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - ACEI/ARB prescription)	2	6027	Odds Ratio (M-H, Random, 95% CI)	1.06 [0.94, 1.19]

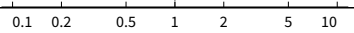
Analysis 1.1. Comparison 1 Behaviours, Outcome 1 Patients appropriately screened (Fordis 2005 - screening for dyslipidaemia; Levine 2011 - LDL measurement).

Study or subgroup	E-learning n/N	Traditional learning n/N	Odds Ratio M-H, Random, 95% CI	Weight	Odds Ratio M-H, Random, 95% CI
Fordis 2005	16/17	17/19		0.41%	1.88[0.16,22.83]
Levine 2011	2711/3080	2594/2911		99.59%	0.9[0.77,1.05]
Total (95% CI)	**3097**	**2930**		**100%**	**0.9[0.77,1.06]**
Total events: 2727 (E-learning), 2611 (Traditional learning)					
Heterogeneity: Tau^2=0; Chi^2=0.34, df=1(P=0.56); I^2=0%					
Test for overall effect: Z=1.29(P=0.2)					

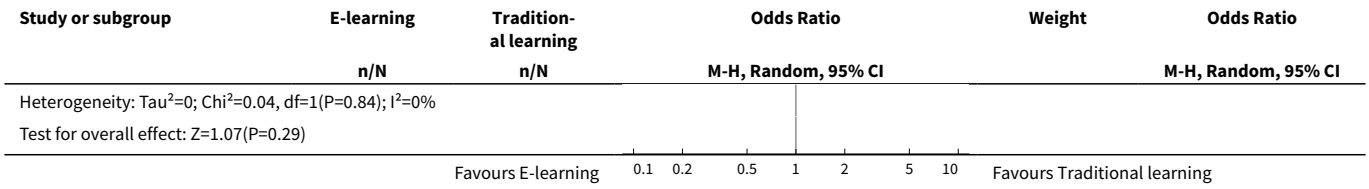
Favours E-learning — Favours Traditional learning

Analysis 1.2. Comparison 1 Behaviours, Outcome 2 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - statin prescription).

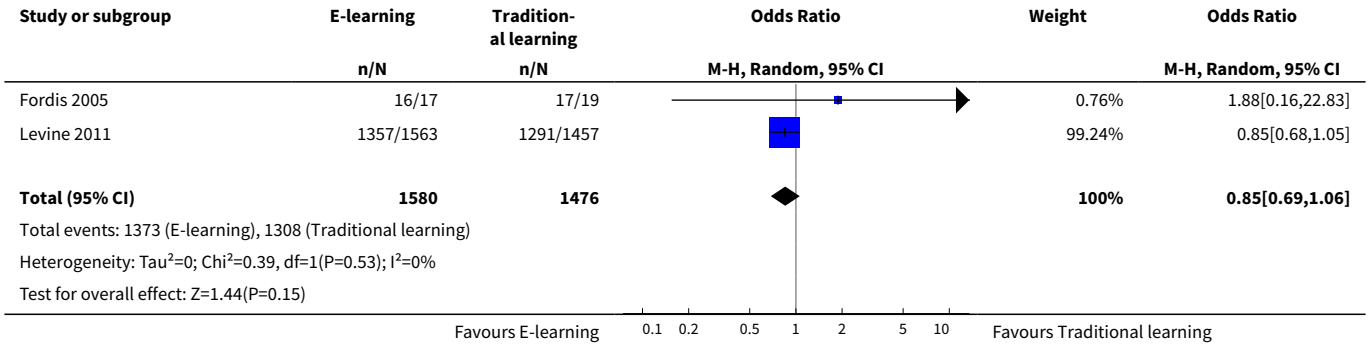
Study or subgroup	E-learning n/N	Traditional learning n/N	Odds Ratio M-H, Random, 95% CI	Weight	Odds Ratio M-H, Random, 95% CI
Fordis 2005	15/17	16/19		1.77%	1.41[0.21,9.62]
Levine 2011	2708/2825	2506/2630		98.23%	1.15[0.88,1.48]
Total (95% CI)	**2842**	**2649**		**100%**	**1.15[0.89,1.48]**
Total events: 2723 (E-learning), 2522 (Traditional learning)					



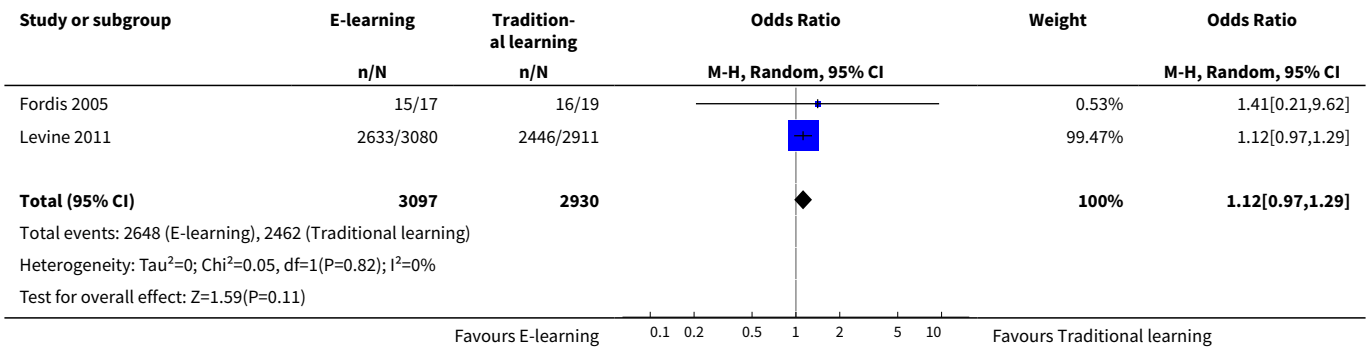
Favours E-learning — Favours Traditional learning



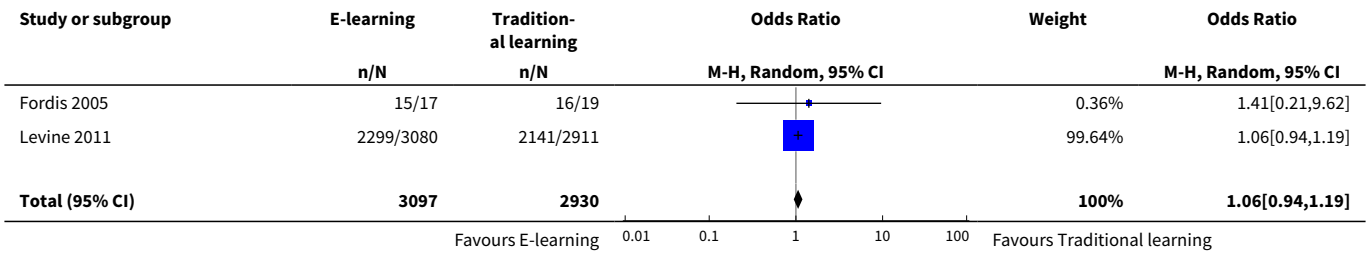
Analysis 1.3. Comparison 1 Behaviours, Outcome 3 Patients appropriately screened (Fordis 2005 - screening for dyslipidaemia; Levine 2011 - HbA1c measurement).

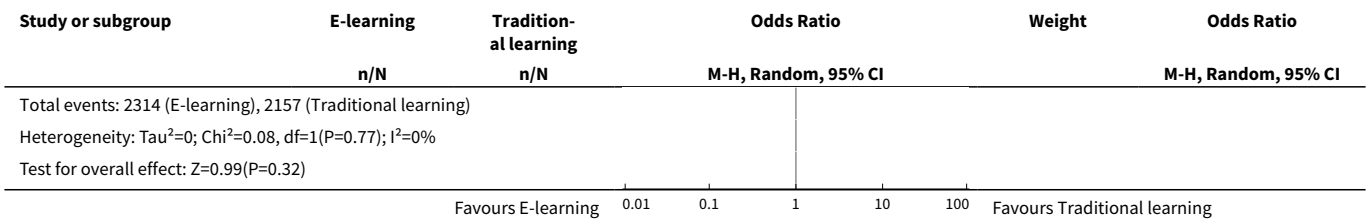


Analysis 1.4. Comparison 1 Behaviours, Outcome 4 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - beta-blocker prescription).



Analysis 1.5. Comparison 1 Behaviours, Outcome 5 Patients appropriately treated (Fordis 2005 - treatment for dyslipidaemia; Levine 2011 - ACEI/ARB prescription).

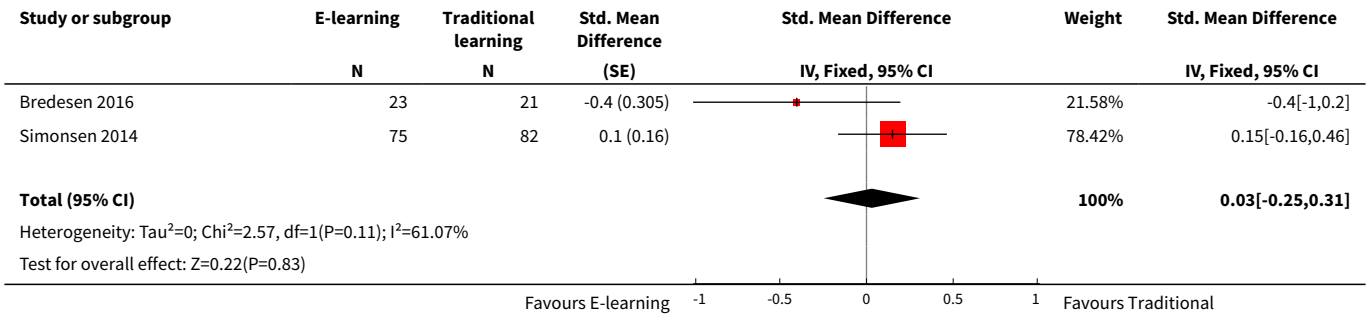




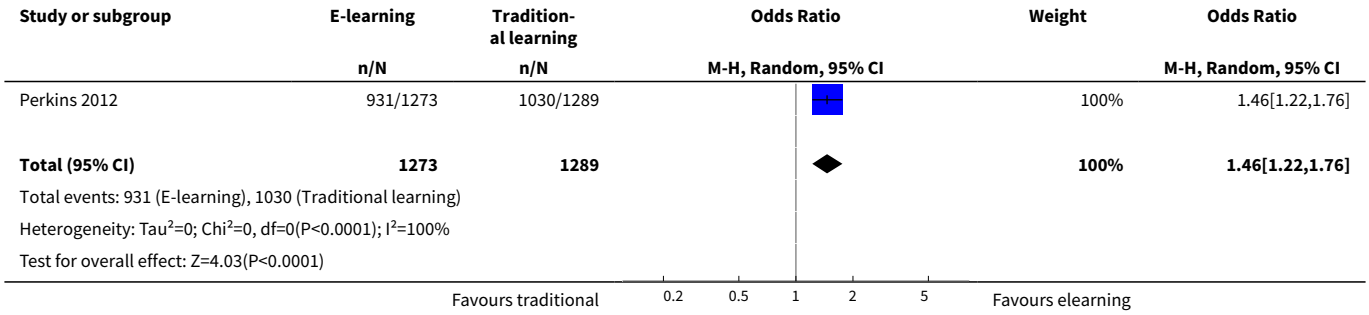
Comparison 2. Skills

Outcome or subgroup title	No. of studies	No. of partici-pants	Statistical method	Effect size
1 Drug dose calculation accuracy (Simonsen 2014); ulcer classification accuracy (Bredesen 2016)	2	201	Std. Mean Difference (Fixed, 95% CI)	0.03 [-0.25, 0.31]
2 Cardiac arrest simulation test (CASTest)	1	2562	Odds Ratio (M-H, Random, 95% CI)	1.46 [1.22, 1.76]

Analysis 2.1. Comparison 2 Skills, Outcome 1 Drug dose calculation accuracy (Simonsen 2014); ulcer classification accuracy (Bredesen 2016).



Analysis 2.2. Comparison 2 Skills, Outcome 2 Cardiac arrest simulation test (CASTest).



Comparison 3. Knowledge

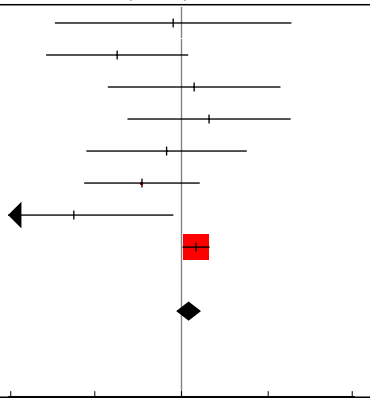
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 At any time (fixed-effect)	8	3082	Std. Mean Difference (Fixed, 95% CI)	0.04 [-0.03, 0.11]
2 At any time (random-effects)	8	3082	Std. Mean Difference (Random, 95% CI)	-0.09 [-0.27, 0.09]
3 Immediately after the training	7	3012	Std. Mean Difference (Random, 95% CI)	-0.10 [-0.29, 0.08]
4 After 3 or more months	3	225	Std. Mean Difference (Random, 95% CI)	-0.07 [-0.41, 0.27]

Analysis 3.1. Comparison 3 Knowledge, Outcome 1 At any time (fixed-effect).

Study or subgroup	E-learning N	Traditional learning N	Std. Mean Difference (SE)	Std. Mean Difference IV, Fixed, 95% CI	Weight	Std. Mean Difference IV, Fixed, 95% CI
Benjamin 2008	17	16	-0 (0.348)		1.1%	-0.05[-0.73,0.63]
Fordis 2005	44	49	-0.4 (0.209)		3.04%	-0.37[-0.78,0.04]
Harris 2008	30	32	0.1 (0.254)		2.06%	0.07[-0.42,0.57]
Horiuchi 2009	37	33	0.2 (0.24)		2.3%	0.16[-0.31,0.63]
Hugenholtz 2008	37	35	-0.1 (0.236)		2.38%	-0.09[-0.55,0.38]
Khatony 2009	70	70	-0.2 (0.17)		4.59%	-0.23[-0.56,0.11]
Paladino 2007	24	25	-0.6 (0.293)		1.55%	-0.62[-1.2,-0.05]
Perkins 2012	1274	1289	0.1 (0.04)		82.98%	0.08[0.01,0.16]
Total (95% CI)					**100%**	**0.04[-0.03,0.11]**

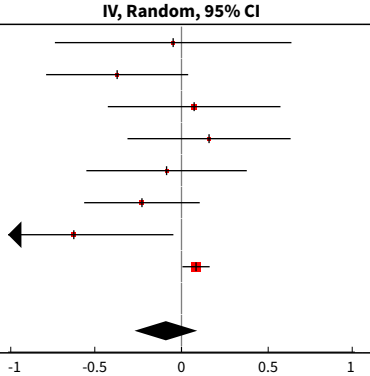
Heterogeneity: Tau^2=0; Chi^2=13.26, df=7(P=0.07); I^2=47.22%

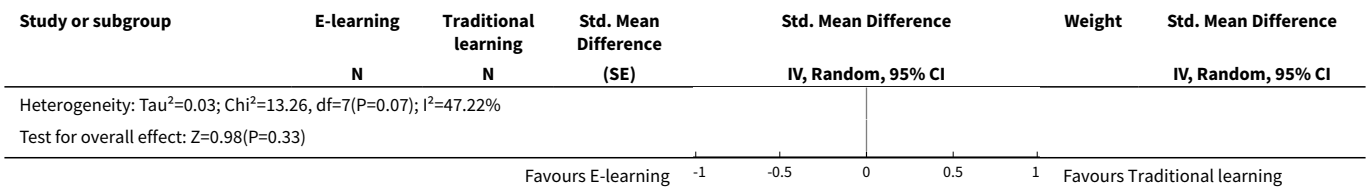
Test for overall effect: Z=1.12(P=0.26)



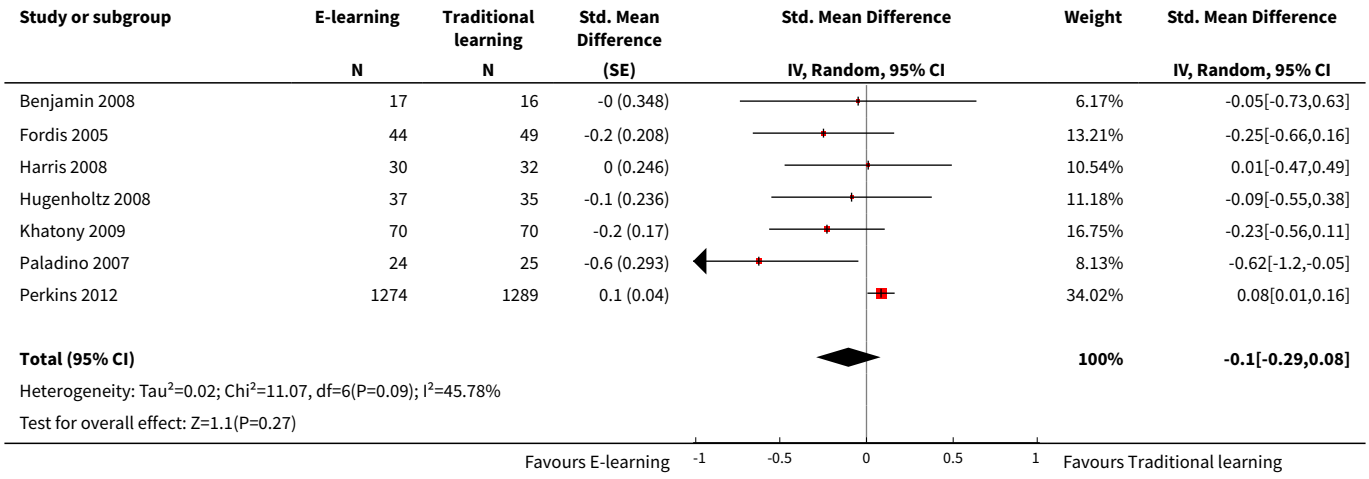
Analysis 3.2. Comparison 3 Knowledge, Outcome 2 At any time (random-effects).

Study or subgroup	E-learning N	Traditional learning N	Std. Mean Difference (SE)	Std. Mean Difference IV, Random, 95% CI	Weight	Std. Mean Difference IV, Random, 95% CI
Benjamin 2008	17	16	-0 (0.348)		5.76%	-0.05[-0.73,0.63]
Fordis 2005	44	49	-0.4 (0.209)		12.05%	-0.37[-0.78,0.04]
Harris 2008	30	32	0.1 (0.254)		9.31%	0.07[-0.42,0.57]
Horiuchi 2009	37	33	0.2 (0.24)		10.07%	0.16[-0.31,0.63]
Hugenholtz 2008	37	35	-0.1 (0.236)		10.3%	-0.09[-0.55,0.38]
Khatony 2009	70	70	-0.2 (0.17)		15.23%	-0.23[-0.56,0.11]
Paladino 2007	24	25	-0.6 (0.293)		7.55%	-0.62[-1.2,-0.05]
Perkins 2012	1274	1289	0.1 (0.04)		29.71%	0.08[0.01,0.16]
Total (95% CI)					**100%**	**-0.09[-0.27,0.09]**

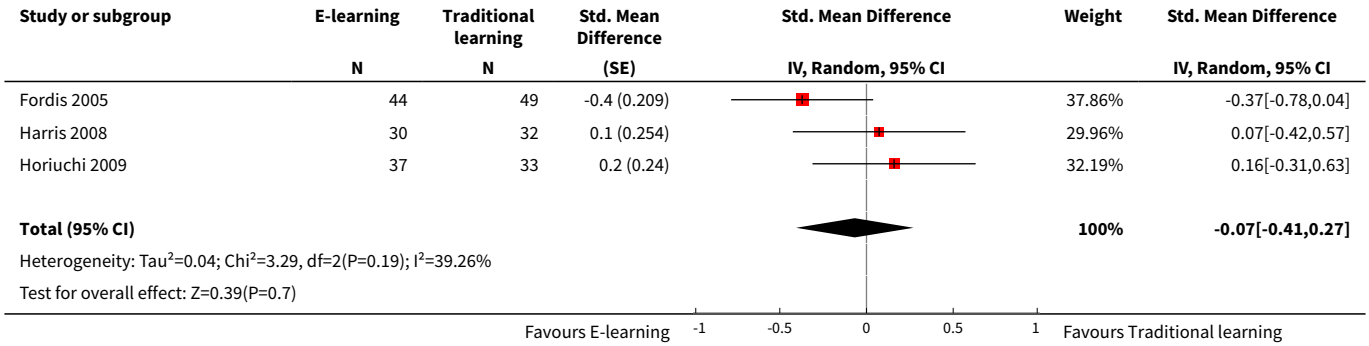




Analysis 3.3. Comparison 3 Knowledge, Outcome 3 Immediately after the training.



Analysis 3.4. Comparison 3 Knowledge, Outcome 4 After 3 or more months.



APPENDICES

Appendix 1. Search strategies

Medline (OVID)

Epub Ahead of Print, In-Process & Other Non-Indexed Citations, Ovid MEDLINE(R) Daily and Ovid MEDLINE(R) 1946 to present

No.	Search terms	Results
1	("e-learning" or elearning).ti.	857
2	("e-learning" or elearning).ab.	1376
3	or/1-2	1662
4	*internet/ and *education/	55
5	((electronic or internet or internet-based or online or "on line" or remote or distance or mobile or web or "web 2*" or web-based or web deliver*) adj2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)).ti,ab.	7437
6	((computeri?ed or computer-assisted or computer-mediated* or computer-based) adj2 (class or classes or classroom? or class-room? or course or courses or coursework or course-work or education or inservice or in-service or instruction* or learning or seminar? or teaching or workshop?)).ti,ab.	1743
7	((e-mail* or email* or e-mail-based or email-based) adj2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)).ti,ab.	83
8	(e-education or e-instruction or elearning or "e learning" or "e train*" or "e curricul*" or "e program*" or m-learn*).ti,ab.	1792
9	(virtual adj2 (class or classes or classroom? or course? or education* or in-service or in-service or instruction* or instructor? or learning or seminar? or teacher? or teaching or training or trainer? or workshop*)).ti,ab.	1243
10	((3g or 4g or ipad or iphone or handheld or (tablet adj5 computer?) or android or cell phone or mobile phone) adj4 (educational or class)).ti,ab.	27
11	(distributed adj3 (curricul* or education or learning)).ti,ab.	298
12	spaced learning.ti,ab.	35
13	("remote course*" or "remote education" or "remote seminar?" or "remote learning" or "remote workshop*" or (remote participation adj4 (education? or workshop or course or learning))).ti,ab.	40
14	(virtual or online or web or internet).ti.	51312
15	or/4-14	59766
16	*postgraduate education/ or *continuing education/ or *in service training/ or *professional development/	3449
17	(post-graduate or graduate education or graduate degree? or ((master? or doctoral) adj2 degree?) or doctorate or doctoral or post-professional).ti,ab.	8089
18	(continuing adj2 (medical or nursing or pharmacist? or physician? or doctor? or allied health) adj3 education?).ti,ab.	5321

(Continued)

19	(inservice training or professional development or cme).ti,ab.	11093
20	or/16-19	26273
21	(15 and 20) not 3	913
22	*nurse/ or exp *paramedical personnel/ or exp *physician/ or *medical personnel/	132064
23	(continuing adj2 education?).ti,ab,hw.	62702
24	(and/15,22-23) not (or/3,21)	77
25	*dental education/ or *medical education/ or *nursing education/	68626
26	25 not (undergraduate? or first year or second year or third year or preclinical or pre-clinical).ti,ab,hw.	63971
27	(26 and 15) not (or/3,21,24)	1166
28	controlled clinical trial/ or controlled study/ or randomized controlled trial/	510348
29	randomi?ed.ti. or ((random* or control) adj3 (group? or cohort? or patient? or hospital* or department?)).ab. or (controlled adj2 (study or trial)).ti.	641737
30	(multicenter and (study or trial)).ti.	20362
31	(random sampl* or random digit* or random effect* or random survey or random regression).ti,ab. not randomized controlled trial/	62344
32	(exp animals/ or exp invertebrate/ or animal experiment/ or animal model/ or animal tissue/ or animal cell/ or nonhuman/) and (human/ or normal human/ or human cell/)	16144262
33	(exp animals/ or exp invertebrate/ or animal experiment/ or animal model/ or animal tissue/ or animal cell/ or nonhuman/) not 32	4275233
34	(or/28-30) not (or/31,33)	841718
35	3 and 34	176
36	21 and 34	58
37	24 and 34	9
38	27 and 34	54
39	or/35-38	297

Embase (OVID)

Embase 1974 to 2016 July 07

No.	Search terms	Results
1	("e-learning" or elearning).ti.	1157
2	("e-learning" or elearning).ab.	2220
3	or/1-2	2597
4	computer-assisted instruction/	62027
5	((electronic or internet or internet-based or online or "on line" or remote or distance or mobile or web or "web 2*" or web-based or web deliver*) adj2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)).ti,ab.	9126
6	((computeri?ed or computer-assisted or computer-mediated* or computer-based) adj2 (class or classes or classroom? or class-room? or course or courses or coursework or course-work or education or inservice or in-service or instruction* or learning or seminar? or teaching or workshop?)).ti,ab.	2086
7	((e-mail* or email* or e-mail-based or email-based) adj2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)).ti,ab.	156
8	(e-education or e-instruction or elearning or "e learning" or "e train*" or "e curricul*" or "e program*" or m-learn*).ti,ab.	2778
9	(virtual adj2 (class or classes or classroom? or course? or education* or in-service or in-service or instruction* or instructor? or learning or seminar? or teacher? or teaching or training or trainer? or workshop*)).ti,ab.	1632
10	((3g or 4g or ipad or iphone or handheld or (tablet adj5 computer?) or android or cell phone or mobile phone) adj4 (educational or class)).ti,ab.	45
11	(distributed adj3 (curricul* or education or learning)).ti,ab.	352
12	spaced learning.ti,ab.	46
13	("remote course*" or "remote education" or "remote seminar?" or "remote learning" or "remote workshop*" or (remote participation adj4 (education? or workshop or course or learning))).ti,ab.	55
14	(virtual or online or web or internet).ti.	59771
15	or/4-14	128433
16	education, medical, continuing/ or education, medical, graduate/ or exp "internship and residency"/ or education, nursing, continuing/ or education, nursing, graduate/ or education, pharmacy, continuing/ or education, pharmacy, graduate/ or pharmacy residencies/ or inservice training/ or staff development/	660488
17	(post-graduate or graduate education or graduate degree? or ((master? or doctoral) adj2 degree?) or doctorate or doctoral or post-professional).ti,ab.	10031

(Continued)

18	(continuing adj2 (medical or nursing or pharmacist? or physician? or doctor? or allied health) adj3 education?).ti,ab.	6614
19	(inservice training or professional development or cme).ti,ab.	15275
20	or/16-19	674033
21	(15 and 20) not 3	49387
22	exp allied health personnel/ or exp *dentists/ or exp medical staff/ or exp nurses/ or pharmacists/ or exp physicians/	907485
23	(continuing adj2 education?).ti,ab,hw.	43200
24	(and/15,22-23) not (or/3,21)	176
25	education, dental/ or education, medical/ or education, nursing/ or education, pharmacy/	537908
26	25 not (undergraduate? or first year or second year or third year or preclinical or pre-clinical).ti,ab,hw.	514219
27	(26 and 15) not (or/3,21,24)	27
28	(randomized controlled trial or controlled clinical trial).pt. or randomized.ab. or placebo.ab. or clinical trials as topic.sh. or randomly.ab. or trial.ti.	981031
29	exp animals/ not humans.sh.	21860327
30	28 not 29	92471
31	(3 or 21 or 24 or 27) and 30	232

The Cochrane Library (Wiley)

No.	Search terms	Results
#1	("e-learning" or elearning):ti	117
#2	("e-learning" or elearning):ab	188
#3	{or #1-#2}	216
#4	[mh "computer-assisted instruction"]	1039
#5	((electronic or internet or internet-based or online or "on line" or remote or distance or mobile or web or "web 2*" or web-based or web deliver*) near/2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)):ti,ab	656
#6	((computeri?ed or computer-assisted or computer-mediated* or computer-based) near/2 (class or classes or classroom? or class-room? or course or	276

(Continued)

	courses or coursework or course-work or education or inservice or in-service or instruction* or learning or seminar? or teaching or workshop?)):ti,ab	
#7	((e-mail* or email* or e-mail-based or email-based) near/2 (class or classes or classroom? or class-room? or course or courses or course-work or education* or inservice or in-service or instruction* or learning or seminar? or teaching or workshop? or work-shop?)):ti,ab	25
#8	(e-education or e-instruction or elearning or "e learning" or "e train*" or "e curricul*" or "e program*" or m-learn*):ti,ab	275
#9	(virtual near/2 (class or classes or classroom? or course? or education* or in-service or in-service or instruction* or instructor? or learning or seminar? or teacher? or teaching or training or trainer? or workshop*)):ti,ab	174
#10	((3g or 4g or ipad or iphone or handheld or (tablet near/5 computer?) or android or cell phone or mobile phone) near/4 (educational or class)):ti,ab	4
#11	(distributed near/3 (curricul* or education or learning)):ti,ab	15
#12	spaced learning:ti,ab	52
#13	("remote course*" or "remote education" or "remote seminar?" or "remote learning" or "remote workshop*" or (remote participation near/4 (education? or workshop or course or learning))):ti,ab	3
#14	(virtual or online or web or internet):ti	5035
#15	{or #4-#14}	6458
#16	[mh "education, medical, continuing"] or [mh "education, medical, graduate"] or [mh "internship and residency"] or [mh "education, nursing, continuing"] or [mh "education, nursing, graduate"] or [mh "education, pharmacy, continuing"] or [mh "education, pharmacy, graduate"] or [mh "pharmacy residencies"] or [mh "inservice training"] or [mh "staff development"]	2528
#17	(post-graduate or graduate education or graduate degree? or ((master? or doctoral) near/2 degree?) or doctorate or doctoral or post-professional):ti,ab	225
#18	(continuing near/2 (medical or nursing or pharmacist? or physician? or doctor? or allied health) near/3 education?):ti,ab	2
#19	(inservice training or professional development or cme):ti,ab	730
#20	{or #16-#19}	3340
#21	(#15 and #20)	339
#22	[mh "allied health personnel"] or [mh *dentists] or [mh "medical staff"] or [mh nurses] or [mh pharmacists] or [mh physicians]	4047
#23	(continuing near/2 education?):ti,ab,kw	2
#24	#15 and #22 and #23	0
#25	[mh "education, dental"] or [mh "education, medical"] or [mh "education, nursing"] or [mh "education, pharmacy"]	3454

(Continued)

#26	#25 not (undergraduate? or first year or second year or third year or preclinical or pre-clinical):ti,ab,kw	2873
#27	#26 and #15	456
#28	#3 or #21 or #24 or #27	720

FEEDBACK

Serious concerns regarding the conduct of this review, 28 May 2018

Summary

The following is a summary of the comments from Dr Penny Whiting and Assoc. Prof Josip Car.

We have serious concerns regarding the conduct of this review for the following reasons:

1. We are aware of eligible studies that are not included in the review (a list was provided by the commenters)

2. Key databases were not searched (e.g. ERIC)

3. No attempts were made to locate unpublished studies

4. There is ambiguity in inclusion criteria – they could be open to manipulation

5. eLearning term definition lacks clarity making it difficult to apply

6. There is lack of clarity in review question

7. The review was restricted to studies that used traditional learning as the comparison. Other comparisons e.g. to other types of eLearning, or blended learning are equally important

8. Insufficient study details are available, especially regarding interventions

9. Methods to pool data are not appropriate (use of fixed effect model when substantial differences between studies); it is questionable whether pooling is appropriate

10. Differences between studies are not adequately considered

11. Interpretation of data should consider the role of eLearning

The commenters also conducted and shared a more detailed assessment of the review using the ROBIS tool and MECIR criteria. This was sent to the review authors who used it to inform and supplement their response to the main points (listed below).

Reply

Reply from Dr Lorenzo Moja on behalf of all authors.

First of all, we would like to thank Whiting and Car for their in-depth analysis and comments on our review, which we will help improve its relevance and quality for future updates. We provide a point by point response to the points raised in their submitted comments. In addition, we have read their expanded comments including the list of potentially eligible studies.

1. We are aware of eligible studies that are not included in the review (list provided)

The research strategy was designed and developed in agreement with the EPOC Group. It is the result of careful work that included several terms that characterise experimental studies on e-learning. The search strategy was tested and calibrated to achieve comprehensiveness of coverage, while maintaining a certain degree of precision. It is possible that the strategy refinements reduced its exhaustiveness. Moreover, the search strategy was developed to select only a specific population (i.e. licensed health professionals) and comparator (i.e. traditional learning) of a broader intervention type. These elements may have made the research strategy less sensitive.

Whiting et al. suggested that we excluded trials in our review that should have been included. They highlight seven trials, which they cite as includable in accordance with our protocol (Vaona A, Rigon G, Banzi R, Kwag KH, Cereda D, Pecoraro V, Moja L, Bonovas

S. E-learning for health professionals (Protocol). Cochrane Database of Systematic Reviews 2015, Issue 6. Art. No.: CD011736. DOI: 10.1002/14651858.CD011736).

With regards to the studies raised by Whiting and Car, we discuss each study, providing our reasons for exclusion. The studies are presented in alphabetical order.

Bell D S, Fonarow G C, Hays R D, Mangione C M. Self-study from web-based and printed guideline materials. A randomized, controlled trial among resident physicians. Annals of internal medicine 2000;132:938-46.

We identified and excluded this study. Participants were 162 residents. Studies in which participants are residents were excluded. As this is clear from the title, this study is reported among excluded studies in the PRISMA flow diagram.

Estrada Carlos A, Safford Monika M, Salanitro Amanda H, Houston Thomas K, Curry William, Williams Jessica H, et al. A web-based diabetes intervention for physician: a cluster-randomized effectiveness trial. International journal for quality in health care: journal of the International Society for Quality in Health Care / ISQua 2011;23:682-9.

We identified and excluded this study. E-learning was part of a multi-component intervention, which also encompassed audit and feedback, an intervention supported by evidence of effectiveness per se. When e-learning was merely added to a multifaceted intervention that could easily be offered in its absence (e.g. audit and feedback interventions), we considered the intervention as 'not core', and excluded the study; this study is reported in the excluded studies section.

Hemmati Nima, Omrani Soghra, Hemmati Naser. A Comparison of Internet-Based Learning and Traditional Classroom Lecture to Learn CPR for Continuing Medical Education. Turkish Online Journal of Distance Education 2013;14:256-65.

We did not identify this study. This study might meet our inclusion criteria. However, as the report of the study describes it as quasi-experimental, it cannot be included before authors confirm that the allocation followed a true randomisation process.

Franchi C, Tettamanti M, Djade C D, Pasina L, Mannucci P M, Onder G, et al. E-learning in order to improve drug prescription for hospitalized older patients: a cluster-randomized controlled study. British Journal of Clinical Pharmacology 2016;82(1):53-63.

We identified and excluded this study. E-learning was used both in the trial intervention and control arms. As such, the study was excluded. In our review, this study is reported in the excluded studies section.

Girgis Afaf, Cockburn Jill, Butow Phyllis, Bowman Deborah, Schofield Penelope, Stojanovski Elizabeth, et al. Improving patient emotional functioning and psychological morbidity: evaluation of a consultation skills training program for oncologists. Patient education and counselling 2009;77:456-62.

This study was not identified by our search strategy. Participants assigned to the control group did not receive any educational intervention. As our inclusion criteria specified trials in which the eligible comparators were educational interventions on the same topic without access to e-learning, we would have excluded this study.

Kerfoot B Price, Turchin Alexander, Breydo Eugene, Gagnon David, Conlin Paul R. An online spaced-education game among clinicians improves their patients' time to blood pressure control: a randomized controlled trial. Circulation. Cardiovascular quality and outcomes 2014;7:468-74.

We identified and excluded this study (list of excluded studies). Two reviewers agreed to exclude the study based on the abstract, which stated that the control arm participants also received an e-learning intervention.

Legare France, Labrecque Michel, Cauchon Michel, Castel Josette, Turcotte Stephane, Grimshaw Jeremy. Training family physicians in shared decision-making to reduce the overuse of antibiotics in acute respiratory infections: a cluster randomized trial. CMAJ: Canadian Medical Association journal 2012;184:E726-34.

This study was not identified by our search strategy. However, participants assigned to the control group did not receive any educational intervention. Moreover, half of the participants were residents.

We have demonstrated that our study selection was not flawed and that inclusion/exclusion was undertaken with sufficient scientific justification. We also provide clear reasons for exclusion to reduce opportunities for potential ambiguities in the eligible criteria.

2 & 3. Key databases were not searched (e.g. ERIC); No attempts to locate unpublished studies.

MECIR divides standards in mandatory and highly desirable. Searching specialist bibliographic databases, for instance, is highly desirable. We have demonstrated that our review did not have any serious methodological flaws in terms of the methods used to identify and/or select studies. However, we acknowledge the value of the ERIC database. Our information scientist has commented "As noted, 6 of these 7 studies are in Medline. The other study is not indexed in any of the sources that were searched, however it is indexed in ERIC, which has been suggested as a subject specific database to search for this review. ERIC will be added as a complementary source to the databases that were already considered in our search strategy. The studies we did not identify will be useful in creating future iterations of the search strategy.

From the detailed comments the commenters say "There are several instances in the search strategy where "not" does not appear to have been appropriately used leading to potentially missed studies". Our information scientist has reviewed the search strategy and cannot identify any instances where the use of "not" in the searches would have inappropriately restricted the results.

Our literature search was comprehensive, but did not include specific efforts to identify unpublished studies. Although it is possible that a certain amount of unpublished studies could be retrieved, we reasoned a priori that large efforts would not be particularly fruitful in this area. Publication bias, such that positive studies have a much larger chance of being published, might not be generalizable to scientific literature focused on medical education.

We made reasonable efforts to recover incomplete or unpublished data. In cases of uncertainty regarding study designs, we contacted the authors of original RCTs to obtain additional information before considering any study for inclusion or exclusion. All emails are reported in the references section. These correspondences were an additional effort to the thorough online search we conducted, because we wanted to make sure that we were inclusive.

The search end date is July 2016. We acknowledge that searches for all relevant databases should be updated within 12 months before publication of the review. An update of the review is ongoing.

We could not find differences between the hits given in the Medline search strategy and the number reported in the flow diagram.

From the detailed comments the commenters say "Is EPOC methodology filter appropriate in addition to RCT filter?" The EPOC information scientist commented "The inclusion criteria is for randomised trials only hence only a study design filter for randomised trials being used in the search. The search methods in the review have now been amended to reflect this."

4, 5, 6 & 7. Ambiguity in inclusion criteria – open to manipulation; eLearning term definition lacks clarity making it difficult to apply; lack of clarity in review question; The review was restricted to studies that used traditional learning as the comparison. Other comparisons e.g. to other types of eLearning, or blended learning are equally important

We do not find our review ambiguous or feel it has been open to manipulation; we reported the inclusion criteria transparently for all readers to access. However, we will consider providing more operationalization details in an update, particularly when we refer to the inclusion of only interventions in which e-learning is a core or essential element. We stated: "in multifaceted educational interventions (e.g. those applying two or more interventions to change health professionals' practice), the e-learning component may have different degrees of centrality. Thus, we categorised studies into three groups: 1. e-learning alone; 2. e-learning as a core, essential component of a multifaceted intervention; 3. e-learning as a component of a multifaceted intervention, but not considered as core and essential." For example, it would add clarity to report that: a) we considered e-learning as core and essential when authors specified the levels of exposure of participants to the e-learning and other interventions, and b) exposure to e-learning was greater as compared to other interventions.

Our inclusion and exclusion criteria as well as our definition of e-learning were thoroughly discussed with internal and external peer-reviewers. As the commenters observe, there is currently no standardized definition of e-learning. We preferred to adopt a wide and pragmatic definition. We are happy to compare our definition with others, particularly if changes in the definition can alter the cumulative evidence of our review.

Our question compares e-learning to traditional learning. We considered that this is the most important question to be answered, as the compared interventions are at the opposite of a spectrum of educational interventions. We decided to include only interventions in which e-learning is considered a core and essential component of the intervention. In doing so, we did decide to privilege simpler mono-component e-learning intervention over complex multi-component interventions. We acknowledge that blended interventions are popular and may be of interest to several readers. However, if a difference exists, this will likely emerge only by comparing very diverse interventions. We agree with the commenters that, given the advantages of e-learning over standard learning in some dimensions (e.g. feasibility), assessing equivalence might be appropriate. However, Cochrane reviews, including their reporting, are standardized around superiority.

We have no interest in manipulating the inclusion/exclusion of single studies, as we have no preconceived preference, or any interest, in one form of learning being superior to the other.

mLearning (mobile learning) could currently be included as "the learners may have had access to interventions through a variety of technologies (e.g. computers, personal digital assistant (PDA), smart phones, etc.)" and no exclusion was made on the basis of the device used to learn.

We believe that medical topics are the most relevant to assess clinical relevance, and to support knowledge and decision making driven by e-learning. Medical topics are not exclusive to physicians, but are the core curricula elements of other health care professionals. For this reason, we excluded non-medical topics, as they would have increased the heterogeneity without providing added relevance. Examples of non-medical topics are hospital business administration, workplace safety, and using PubMed tutorials. We regarded the differentiation between non-medical and medical topics to be intuitive.

Finally, we considered guideline availability or dissemination as a form of traditional learning. These types of controls were accordingly considered as includable.

8. Insufficient study details available, especially regarding interventions

We collected detailed information about the interventions. Our initial tables were, in fact, more detailed to the point of being judged cumbersome. Within the editorial process, we had to compromise between succinct and more readable versus longer and more comprehensive descriptions. We agreed with the suggestion of editors and reviewers to limit the length of the Characteristics of included studies.

The "other risk of bias" is related to the "conflict of interest," and this additional dimension is presented in the Risk of Bias tables.

9. Methods to pool data not appropriate (use of fixed effect model when substantial differences between studies), questionable whether pooling appropriate

We followed a sound methodology for estimating the effect size across studies. We did not present a fixed effects model only but have presented in the text random effects for our primary outcome (i.e. behaviors), and we acknowledged both fixed and random effects models for an a priori secondary outcome (i.e. knowledge), allowing the reader to compare the results of different meta-analytic models.

We deemed appropriate the use of a fixed effect for the knowledge outcome analysis. Eight studies (3082 participants) were meta-analysed. We faced an unusual situation of the analysis being dominated by a single large trial (2563 participants; SMD 0.08, 95% CI 0.01 to 0.16) at low risk of bias in slight favor of traditional learning. All other studies were small and at high risk of bias. Overall, the observed heterogeneity was moderate (I^2=47%). Our decision to preference the fixed effect model was based on the following considerations: i) our inclusion and exclusion criteria are narrow, so we are confident the studies we selected are sufficiently similar; ii) evaluation of risk of bias is a pillar of Cochrane systematic reviews; if studies are at different risks of bias, studies at low risk of bias should be preferred; and iii) the choice between a fixed-effect and a random-effects meta-analysis should never be made on the basis of a statistical test for heterogeneity. In the random effects model, the weight of Perkins falls from 83% to 29.7%. The small studies gain between 100% to 300% informative power.

Initially, the review reported the results of both fixed and random effect models. However, the results of analyses, and their general interpretations, were not dissimilar. The difference between e-learning and traditional learning is minimal under both models (fixed effect SMD 0.04, 95% CI -0.03 to 0.11; random-effects SMD -0.09, 95% CI -0.27 to 0.09). One of the reviewers noted "ultimately the conclusion remains largely the same: that overall they [the authors] did not detect a difference between e-learning and non-e-learning". The certainty of the evidence was rated as low.

It is worth noting that Higgins and Spiegelhalter discussed a very similar meta-analytic scenario in 2002 – one large trial and several small trials – and the opportunity to use the fixed effects and random effects models (Higgins JP, Spiegelhalter DJ. Being sceptical about meta-analyses: a Bayesian perspective on magnesium trials in myocardial infarction. Int J Epidemiol. 2002;31(1):96-104). The dispute about the superiority of one model to the other was unsolvable, with reasons in both sides.

10. Differences between studies not adequately considered

The vast majority of meta-analyses attempt to cumulate study results even when these are precarious and stretched in the face of large heterogeneity. The most cited meta-analysis on e-learning included and cumulated quasi-experimental designs, such as uncontrolled before-and-after designs (more than half in the Internet-based learning vs no intervention comparison), and experimental studies (Cook DA, Levinson AJ, Garside S, Dupras DM, Erwin PJ, Montori VM. Internet-based learning in the health professions: a meta-analysis. JAMA. 2008;300 (10):1181-1196). *In our meta-analyses, the conceptual, methodological and statistical heterogeneities are more limited. For instance, all included studies adopt the same design, i.e. RCT. Since we adopted strict inclusion criteria, characteristics of studies are similar. Nevertheless, our meta-analyses are bound to have studies that slightly differ with reference to PICO dimensions, e.g. outcomes might be measured at different time points. Unfortunately, the small number of studies included limited our ability to investigate heterogeneity using various sub-group analyses and meta-regressions, shedding light on what can be an effect modifier of study effect.*

11. Interpretation of data should consider role of eLearning

We think that the comment makes an important point: our review includes only RCTs, and the objective is to contrast e-learning versus traditional learning. Any difference between the intervention and control arms can be assumed to be caused by e-learning. When the sample is sufficiently large to exclude important differences, e-learning and traditional learning could be assumed to provide similar benefits. Whiting and Car correctly pointed out that "I would have thought that eLearning being as effective as traditional learning would be what needs to be shown for eLearning to be recommended given the other benefits of eLearning". The reporting of EPOC reviews is standardized, so we had to use the language as per EPOC recommendations (Cochrane Effective Practice and Organisation of Care (EPOC). Reporting the effects of an intervention in EPOC reviews. EPOC Resources for review authors, 2018. Available at: http://epoc.cochrane.org/epocspecific-resources-review-authors). T*he standardized EPOC language has been developed where it is hypothesized that an experimental treatment is superior to a comparison treatment. The same semantic penalizes attempts to determine if the effects of two interventions are not clinically and statistically different from each other. We hope that standardized EPOC language will be revised encompassing cases in which 'therapeutic' equivalence can be hypothesized and discussed. We finally remark that potential advantages of e-learning in dimensions other than those considered by the review, despite not being formally analyzed, are addressed in the introduction and discussion sections.*

Contributors

Dr Penny Whiting (comment author), University of Bristol.

Assoc. Prof Josip Car (comment author), affiliated to Centre for Population Health Sciences, LKC Medicine, NTU Singapore and Global eHealth Unit, Department of Primary Care and Public Health, School of Public Health, Imperial College London, which conduct eLearning research and hold (non-industry) grants to support this work.

Lorenzo Moja (review author)

Paul Miller (EPOC information specialist)

Martin Eccles (EPOC feedback editor)

WHAT'S NEW

Date	Event	Description
7 August 2018	Feedback has been incorporated	Minor amendment to incorporate feedback received 28-May-2018 and the review authors responses. Minor amendment also to the text of the Electronic searches to clarify the methods used.

HISTORY

Protocol first published: Issue 6, 2015
Review first published: Issue 1, 2018

Date	Event	Description
25 April 2018	Amended	Post publication, a study was identified as potentially relevant to the review. This study has been added to 'Studies awaiting classification'.
18 November 2009	Amended	Title change from *E-learning for improving professional practice and patient outcomes* to *E-learning for postgraduate health professionals. W*e restricted the population of interest. This review shares the section dedicated to methods with another systematic review protocol focusing on *E-learning for undergraduate health professionals*.
25 June 2008	Amended	Title change from *E-learning for improving professional practice and patient outcomes* to *E-learning for undergraduate and postgraduate health professionals.*

CONTRIBUTIONS OF AUTHORS

Conception of the study	Cochrane Review Group
Design	LM, RB, DC
Coordinator of the working group and Contact Author	AV
Draft the protocol	AV, LM, RB, VP
Develop and run the search strategy	Trial Search Coordinator

Obtain copies of studies	AV
Revise each draft (text-references ...)	AV
Revise the references and tables	GR, AV
Enter data into RevMan 5 (text)	AV, IT
Enter data into RevMan 5 (references)	AV, IT
Preparation of data sheet for data studies	AV, RB
Select which studies to include	AV, RB, VP, GR, KK, DC
Extract data from studies	AV, RB, VP
Enter data into data sheet	AV, RB, DC
Carry out the analysis	AV, IT, LM
Interpret the analysis	AV, IT, LM
Draft the final review	AV, IT, LM, RB
Update the review	All the authors

DECLARATIONS OF INTEREST

AV: none known.

RB: none known.

KK: none known.

GR: none known.

DC: none known.

VP: none known.

IT: none known.

LM: none known.

SOURCES OF SUPPORT

Internal sources

- EPOC Cochrane Review Group - Editorial base, The Centre for Practice Changing Research, Ottawa Hospital Research Institute (OHRI), Ottawa, Canada.

External sources

- No external source of support, Other.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We changed the protocol title 'E-learning for post-graduate health professionals' into 'E-learning for health professionals' as in many countries health professionals include postgraduate trainees (e.g. residents and fellows), and many trainees are fully licensed. The protocol title might therefore have generated confusion on the target population.

In terms of search strategies, we did not:

- screen individual journals and conference proceedings (e.g. handsearch);
- contact researchers with expertise relevant to the review topic or EPOC interventions (EPOC 2002);
- conduct cited reference searches for all included studies in citations indexes.

We decided to aggregate studies at unclear risk of bias with those at high risk of bias in the sensitivity analysis. We adopted a conservative approach, assuming that the absence of information indicated inadequate quality ('guilty until proven innocent').

Measures of treatment effect: we replaced change scores as the main outcome measures with final scores because we believed that randomisation would adequately prevent differences between experimental and control group baseline scores.

In the protocol we stated, "We took contextual heterogeneity into account and conducted the analyses in subgroups including studies with similar clinical and methodological characteristics: designs, settings, interventions, comparators, outcome scales, effect sizes". This was a misprint, as the sentence was part of a previous draft written when we were still considering also including non-randomised studies.

Changes in the authorship of this Cochrane Review: Irene Tramacere replaced Stefanos Bonovas as statistician.

We decided to perform subgroup analyses if at least 10 observations were available for each characteristic modelled (Higgins 2011a).

INDEX TERMS

Medical Subject Headings (MeSH)

*Internet; Clinical Competence; Education, Distance [*methods]; Health Personnel [*education] [statistics & numerical data]; Randomized Controlled Trials as Topic

MeSH check words

Humans